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ART. XXVII.—*The History of Young's Discovery of his Theory of Colors;* by ALFRED M. MAYER.

THE object of this communication is twofold: I desire first to give complete abstracts from the writings of Newton, Young and Wollaston, in order to put the student of science in possession of all of the early literature of Young's celebrated theory of colors. In the second place, I propose to trace the curious history of the steps by which Young was led to the final adoption of what is now known as Young's theory of color-sensation. In accomplishing the first of these objects, I shall, at the same time, attempt to show, 1st, that Young first formed an hypothesis similar to that known as Brewster's; that is, he selected red, yellow and blue as the three simple color-sensations; 2d, that he subsequently modified his hypothesis and adopted red, green and violet as the three elementary color-sensations, showing that up to the date of this change of opinion all of his ideas on the subject were hypothetical, and not based on any observations or experiments of his own or of others; 3d, that this change of opinion as to the three elementary colors was made on the basis of a misconception by Wollaston of the nature of his celebrated observation of the dark lines in the solar spectrum, and also on the basis of an erroneous observation made by Young in repeating Wollaston's experiment; 4th, that Young subsequently tested his hypothesis of color-sensation and found that it was in accord with facts reached by

experiment; and that these experiments then vindicated his hypothesis and raised it to the dignity of a theory.*

Before discussing the subject proper of this article, it may be well to give the reader a clear conception of Young's theory of color, and to show in what high estimation it is at present held by men of science. This can best be done by the reading of the following short extracts from Helmholtz's "Physiological Optics" and from his "Popular Scientific Lectures."

"To speak of three fundamental colors in an objective sense would be nonsense; in fact, as long as one refers only to purely physical conditions, and while there is no reference to the human eye, the properties of compound light depend alone on the proportions in which exist lights of different wave-lengths. The reduction of light to three fundamental colors can never have anything else than a subjective signification; it consists simply in reducing all *color-sensations* to three fundamental sensations. It is in this sense that Young understood the problem, and his hypothesis gives, in fact, an exceedingly clear and simple explanation and summary of all of the phenomena found in the physiological study of colors. Young states that:

"'1. There exist in the eye three kinds of nerve fibers whose excitation respectively gives the sensation of red, of green, and of violet.

"'2. Homogeneous light excites the three kinds of nerve fibers with an intensity which varies with its wave-length.' That which possesses the greatest length of wave excites most powerfully the fibers sensitive to red, that which has an average wave-length excites the nerves sensitive to green, while that light formed of the shortest waves acts on the fibers which give the violet sensation. Nevertheless we cannot deny, but rather should admit for the explanation of numerous phenomena, that each color of the spectrum excites all three kinds of nerve fibers, but with different intensities. Imagine the colors of the spectrum arranged horizontally in going from the red, R, to the violet, V, as shown at the base of figure 1. The three curves will then represent more or less exactly the degrees of irritability of the three kinds of nerve fibers (1, the red; 2, the green; 3, the violet) for the various colors of the spectrum.

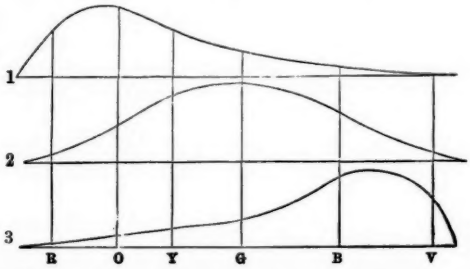
* These two terms, *hypothesis* and *theory*, are so generally misunderstood and thoughtlessly used that it may be well here to give two concise definitions; the first is by Flourens; the second is by Prof. J. Henry.

"An *hypothesis* is the explanation of facts by *possible causes*; a *theory* is the explanation of facts by *real causes*."

"A supposition or guess thus made from analogy as to the nature of the law of a class of facts, is usually called an *hypothesis* and sometimes the *antecedent probability*. When an hypothesis of this kind has been extended and verified, or, in other words, when it has become an exact expression of the law of a class of facts, it is then called a *theory*."

"Pure *red* excites strongly the fibers sensitive to red, and feebly the two other kinds of fibers; sensation, red.

1.



"Pure *yellow* excites moderately the fibers sensitive to red and to green, feebly those fibers sensitive to violet; sensation, yellow.

"Pure *green* excites strongly the fibers sensitive to green, feebly those sensitive to red and to violet; sensation, green.

"Pure *blue* excites in a moderate degree those fibers sensitive to green and to violet, feebly those sensitive to red; sensation, blue.

"Pure *violet* excites strongly those fibers specially destined to receive this sensation, and the other fibers are feebly affected by this light; sensation, violet.

"The nearly equal excitation of all of the fibers will give the sensation of white, or of whitish colors."

* * * * "The choice of the three fundamental colors is to some extent arbitrary. We can choose at will any three colors whose mixture produces white. *Young no doubt was guided by the consideration that the extreme colors of the spectrum occupied the privileged positions.** If we do not choose these colors, we must take for one of the colors a purple tint, and the curve which responds to it in the figure will have two maxima: one in the red, the other in the violet. The hypothesis, without being an impossible one, will be more complicated. As far as I know of, there exists no means of determining directly the fundamental colors but the examination of persons affected with color blindness. We will subsequently see how far that examination confirms the hypothesis of Young, at least so far as the red is concerned."

* * * "In general, then, light which consists of undulations of different wave-lengths produces different impressions upon our eye, namely, those of different colors. But the number of hues which we can recognize is much smaller than that of the various possible combinations of rays with different wave-lengths which external objects can convey to our eyes. The

* The writer has italicized the above for the purpose of a future reference to it.

retina cannot distinguish between the white which is produced by the union of scarlet and bluish-green light, and that which is composed of yellowish-green and violet, or of yellow and ultramarine blue, or of red, green and violet, or of all the colors of the spectrum united. All these combinations *appear* identically as white; and yet from *a physical point of view* they are very different. In fact, the only resemblance between the several combinations just mentioned is, that they are indistinguishable to the human eye. For instance, a surface illuminated with red and bluish-green light would come out black in a photograph; while another lighted with yellowish-green and violet would appear very bright, although both surfaces alike seem to the eye to be simply white.

* * * "Other colors, also, especially when they are not strongly pronounced, may, like pure white light, be composed of very different mixtures, and yet appear indistinguishable to the eye, while in every other property, physical or chemical, they are entirely distinct.

* * * "The theory of colors, with all these marvelous and complicated relations, was a riddle which Goethe in vain attempted to solve; nor were we physicists and physiologists more successful. I include myself in the number; for I long toiled at the task, without getting any nearer my object, until I at last discovered that a wonderfully simple solution had been discovered at the beginning of this century, and had been in print ever since for any one to read who chose. This solution was found and published by the same Thomas Young who first showed the right method of arriving at the interpretation of Egyptian hieroglyphics. He was one of the most acute men who ever lived, but had the misfortune to be too far in advance of his contemporaries. They looked on him with astonishment, but could not follow his bold speculations, and thus a mass of his most important thoughts remained buried and forgotten in 'The Transactions of the Royal Society,' until a later generation by slow degrees arrived at the rediscovery of his discoveries, and came to appreciate the force of his argument and the accuracy of his conclusions."

The first publication by Young of his theory of color appeared in a Bakerian Lecture entitled, "On the Theory of Light and Colors," which Young read before the Royal Society on Nov. 12, 1801. In the opening part of this lecture he says: "A more extensive examination of Newton's various writings has shown me that he was, in reality, the first who suggested such a theory [*the undulatory theory of light*], as I shall endeavor to maintain; that his own opinions varied less from this theory than is now almost universally supposed; and that a variety of arguments have been advanced, as if to confute him, which may be found

in a similar form in his own works; and this by no less a mathematician than Leonard Euler, whose system of light, as far as it is worthy of notice, either was, or might have been, wholly borrowed from Newton, Hooke, Huyghens and Malebranche.

"Those who are attached, as they may be with the greatest justice, to every doctrine which is stamped with the Newtonian approbation, will probably be disposed to bestow on these considerations so much the more of their attention as they shall appear to coincide more nearly with Newton's opinion. For this reason, after having briefly stated each particular position of my theory, I shall collect, from Newton's various writings, such passages as seem to be the most favorable to its admission; and although I shall quote some papers which may be thought to have been partly retracted at the publication of the optics, yet I shall borrow nothing from them that can be supposed to militate against his maturer judgment."

The fact that Young, the *founder* of the undulatory theory of light, in this Bakerian Lecture, in which it has been said that he laid the foundations of that doctrine, should set forth his views in a series of postulates followed by citations from the writings of Newton, to give them weight and proof, may justly surprise those who have trusted to the second-hand information derived from carelessly-complied text books and from hastily-prepared popular lectures. But then, where would be the pugilistic charm of the popular lecturer on the undulatory theory of light, if Newton, his champion, the violent defender of the emanation cause, should decline to enter as a contestant?

Under the heading of Hypothesis III, of this paper, we first meet Young's theory of color-sensation.

"Hypothesis III. *The Sensation of different Colours depends on the different frequency of Vibrations, excited by Light in the Retina.*

Passages from Newton.

"The objector's hypothesis, as to the fundamental part of it, is not against me. That fundamental supposition is, that the parts of bodies, when briskly agitated, do excite vibrations in the ether, which are propagated every way from those bodies in straight lines, and cause a sensation of light, by beating and dashing against the bottom of the eye, something after the manner that vibrations in the air cause a sensation of sound, by beating against the organs of hearing. Now, the most free and natural application of this hypothesis to the solution of phenomena I take to be this: that the agitated parts of bodies, according to their several sizes, figures and motions, do excite vibrations in the ether of various depths or bignesses, which being promiscuously propagated through that medium to our eyes, effect

in us a sensation of light of a white color; but if by any means those of unequal bigness be separated from one another, the largest beget a sensation of a red color, the least or shortest of a deep violet, and the intermediate ones of intermediate colors; much after the manner that bodies, according to their several sizes, shapes, and motions, excite vibrations in the air, of various bignesses, which, according to those bignesses, make several tones in a sound; that the largest vibrations are best able to overcome the resistance of a refracting superficies, and so break through it with least refraction; whence the vibrations of several bignesses, that is, the rays of several colors, which are blended together in light, must be parted from one another by refraction, and so cause the phenomena of prisms and other refracting substances; and that it depends upon the thickness of a thin transparent plate or bubble whether a vibration shall be reflected at its further superficies, or transmitted; so that, according to the number of vibrations interceding the two superficies, they may be reflected or transmitted for many thicknesses. And, since the vibrations which make blue and violet are supposed shorter than those which make red and yellow, they must be reflected at a less thickness of the plate: which is sufficient to explicate all the ordinary phenomena of those plates or bubbles, and also of all natural bodies whose parts are like so many fragments of such plates. These seem to be most plain, genuine, and necessary conditions of this hypothesis. And they agree so justly with my theory, that if the animadversor think fit to apply them, he need not, on that account, apprehend a divorce from it. But yet, how he will defend it from other difficulties, I know not." (Phil. Trans., vii, 5088; Abr. I, 145, Nov., 1672.)

"To explain colors, I suppose, that as bodies of various sizes, densities, or sensations, do, by percussion or other action, excite sounds of various tones, and consequently vibrations on the air of different bigness; so the rays of light, by impinging on the stiff refracting superficies, excite vibrations in the ether—of various bigness; the biggest, strongest, or most potent rays, the largest vibrations; and others shorter, according to their bigness, strength, or power; and therefore the ends of the capillamenta of the optic nerve, which pave or face the retina, being such refracting superficies, when the rays impinge upon them, they must there excite these vibrations, which vibrations (like those of sound in a trunk or trumpet) will run along the aqueous pores or crystalline pith of the capillamenta, through the optic nerve into the sensorium; and there, I suppose, affect the sense with various colors, according to their bigness and mixture; the biggest with the strongest colors, reds and yellows; the least with the weakest, blues and violets; the middle with

green; and a confusion of all with white, much after the manner that, in the sense of hearing, nature makes use of aerial vibrations of several bignesses, to generate sounds of divers tones; for the analogy of nature is to be observed." (Birch, iii, 262, Dec., 1675.)

"Considering the lastingness of the motions excited in the bottom of the eye by light, are they not of a vibrating nature? Do not the most refrangible rays excite the shortest vibrations, —the least refrangible the largest? May not the harmony and discord of colors arise from the proportions of the vibrations propagated through the fibers of the optic nerve into the brain, as the harmony and discord of sound arise from the proportions of the vibrations of the air?" (Optics, Qu. 16, 13, 14.

After these quotations from Newton, Young brings out his hypothesis of color-sensations, under the following:

"*Scholium.* Since, for the reason here assigned by Newton, it is probable that the motion of the retina is rather of a vibratory than of an undulatory nature, the frequency of the vibrations must be dependent on the constitution of this substance. Now, as it is almost impossible to conceive each sensitive point of the retina to contain an infinite number of particles, each capable of vibrating in perfect unison with every possible undulation, it becomes necessary to suppose the number limited; for instance, *to the three principal colors, red, yellow and blue*, of which the undulations are related in magnitude nearly as the numbers 8, 7 and 6; and that each of the particles is capable of being put in motion, less or more forcibly, by undulations differing less or more from a perfect unison; for instance, the undulations of green light, being nearly in the ratio of $6\frac{1}{2}$, will affect equally the particles in unison with yellow and blue, and produce the same effect as a light composed of those two species; and each sensative filament of the nerve may consist of three portions, one for each principal color."

An attentive perusal of the above quotation will show that Young's hypothesis imagines each sensitive point of the retina to contain *particles* capable of vibrating in perfect unison to those vibrations causing three principal colors (red, yellow and blue, in this the first publication of his *hypothesis*), "and that each of the particles is capable of being put in motion, less or more forcibly, by undulations differing less or more from a perfect unison." This would suppose such a triple *molecular* constitution of each nerve fibril as to cause the three species of its constituent molecules (or the atoms forming the molecules) to be *in tune* with the three rates of vibration corresponding respectively to the undulations of the ether causing red, yellow and blue. He afterward says: "and each sensitive filament of the nerve may consist of three portions, one for each principal

color." We have here a conception of the *mode of action* of an ætherial vibration on the retinal nerve fibrils which has not been described by those who have given accounts of Young's theory of color-sensation. Before reading the celebrated Bakerian Lecture, the only knowledge I had directly obtained of Young's theory was from the reading of the account of it as published in vol. i, page 439, of his "Lectures on Natural Philosophy and the Mechanical Arts," London, 1807. This account, however, contains no mention of the physiological part of his theory, and last October I published in the American Journal of Science my paper No 6 of Researches in Acoustics, in which I expressed similar views to those just quoted from Young, as follows:

"For, has modern histology given us any facts concerning the structure of the human retina which point to the establishment of Young's hypothesis of three distinct sets of retinal nerve terminations? The more we study the minute structure of the retinal rods and cones, the farther appears to remove an understanding of the mode of operation of the sensory apparatus of the eye. May not research in this direction be guided by the hypothesis that the *molecular* constitution of the retinal rods and cones is such that their molecules are severally tuned to the vibrations corresponding to the colors red, green and violet? This would lead us to look for effects of actinism on the retina as showing the link existing between the transmitting and sensory functions of the eye. Do not the facts of the known persistence of chemical action, after it has been once initiated, and the time which would be required for the retinal molecules to recombine, or rearrange themselves, after the ætherial vibrations had ceased, comport with the known durations of the residual visual sensations, and with the main facts of physiological optics, better than the hypothesis that masses of the retinal elements are set in vibration, rather than their molecules?"

It requires no argument, it is evident, that the statements made by Young in the foregoing paper, concerning his color hypothesis, were entirely hypothetical, not having been based on any observation or experiment either of his own or of others.

The next publication by Young on his theory of color takes place in the following year, and is contained in the following short paragraph, incidentally written toward the conclusion of a paper read by him before the Royal Society, on July 1, 1802, and entitled "An account of some cases of the production of colours, not hitherto described."

"In consequence of Dr. Wollaston's correction of the description of the prismatic spectrum, compared with these observations, it becomes necessary to modify the supposition that I advanced in the last Bakerian lecture, respecting the propor-

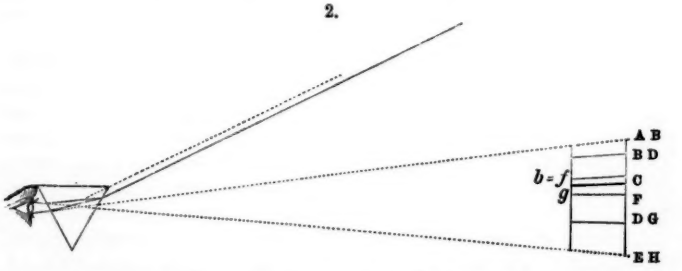
tions of the sympathetic fibers of the retina; substituting red, green, and violet, for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6."

It thus appears that Young changed his three elementary color-sensations from red, yellow, and blue, to *red*, *green*, and *violet*, "in consequence of Dr. Wollaston's correction of the description of the prismatic spectrum." In order to understand fully the ground for this modification of his hypothesis one will be obliged to read the following abstract from Dr. Wollaston's paper, published in "The Transactions of the Royal Society" for 1802; and we are pleased to detain the reader with this paper because it contains the first publication of the observation of those dark lines to which modern spectroscopic research has given such important and prominent significance.

"I cannot conclude these observations on dispersion without remarking that the colours into which a beam of white light is separable by refraction, appear to me to be neither 7, as they usually are seen in the rainbow, nor reducible by any means (that I can find) to 3, as some persons have conceived; but that, by employing a very narrow pencil of light, four primary divisions of the prismatic spectrum may be seen, with a degree of distinctness that, I believe, has not been described nor observed before.

"If a beam of daylight be admitted into a dark room by a crevice $\frac{1}{20}$ of an inch broad, and received by the eye at the distance of 10 to 12 feet through a prism of flint glass, *free from veins*, held near the eye, the beam is seen to be separated into the four following colours only, red, yellowish green, blue and violet; in the proportions represented in fig. 2.

2.



"The line A that bounds the red side of the spectrum is somewhat confused, which seems in part owing to want of power in the eye to converge red light. The line B, between red nd green, in a certain position of the prism, is perfectly distinct; so also are D and E, the two limits of violet. But C,

the limit of green and blue, is not so clearly marked as the rest; and there are also, on each side of this limit, other distinct dark lines, *f* and *g*, either of which, in an imperfect experiment, might be mistaken for the boundary of these colours.

"The position of the prism in which the colours are most clearly divided is when the incident light makes about equal angles with two of its sides. I then found that the spaces A B, B C, C D, D E, occupied by them, were nearly as the numbers 1 6, 2 3, 3 6, 2 5."

In the light of the subsequent careful examinations of the spectrum made in 1814–15 by the celebrated optician Fraunhofer of Munich, we can ascertain what Wollaston really saw. Wollaston, in short, only observed *imperfectly* the dark lines of the spectrum, now known as Fraunhofer's lines, but he *imagined* he saw a spectrum so pure, that is, formed by such a degree of dispersion, that it became divided into four distinct and separated "primary divisions." He at once inferred, and erroneously, that Newton's analysis of the sun's light was false; that no *orange* or *yellow* exists in the spectrum, but between the red and the blue there exists only a "yellowish green." Further on we shall see how Young made a similar but even greater error in his description of this observation. I imagine that when Wollaston's sharp eye caught the glimpse of the divided spectrum, he naturally thought he saw in those divisions uniform colors. It was a natural mistake, and only too readily made, by reason of his mind imagining that the dark lines were *the dividing lines* of the pure simple colors of the solar spectrum.

In the figure illustrating Wollaston's observation, I have placed alongside of his lines A, B, *f*, *g*, D, and E, the corresponding Fraunhofer letters B, D, *b*, F, G, H, thus giving the reader a clear idea of what Wollaston really saw. Wollaston's line "B, between the red and the green," and which he says "in a certain position of the prism is perfectly distinct," we at once recognize as the dark solar line D, occupying a position between the orange and yellow, and well know to be caused by the reversal of the bright yellow light of sodium vapor. No one, however, could now say, after an examination of the spectrum as observed by Wollaston, that the line D divides the red from the green. Wollaston also calls his D and E lines (the G and H lines of Fraunhofer) as "the two limits of the violet;" we now know that G is really on the indigo and that H is within the limits of the violet.

For a more satisfactory comparison of the colors of the solar spectrum as observed by Wollaston and Fraunhofer, I give below the following table. Fraunhofer's results are taken from his colored figure of the spectrum. Both spectra are from flint glass, and their lengths are supposed divided into 360 equal parts.

	Fraunhofer.	Wollaston.	
Red,	56	57·6	Red.
Orange,	27		
Yellow,	27		
Green,	46	82·8	Yellowish green.
Blue,	48	129·6	Blue.
Indigo,	47		
Violet,	109	90	Violet.
	360	360	

Fraunhofer's observations are irreproachable, and are to this day in high esteem for their accuracy. They were made by placing the prism in front of a telescope, mounted on a divided horizontal circle; and viewing a distant slit through the prism and telescope, he observed spectra as pure as those given by modern spectroscopes of low power. Fraunhofer discerns orange and yellow and green where Wollaston only sees yellowish green. Also, Wollaston did not see all of the violet, as we might suspect from his having bounded its upper limits by the line H. Fraunhofer saw 109 parts of violet, Wollaston only 90.

The above discussion, I think, has clearly shown that Wollaston made a false interpretation of his observation in supposing that he had discovered a pure spectrum naturally divided by dark lines into four simple colors, and that he also erred in the proportions which he relatively gave to them. Also, I have shown that Young, in finally selecting red, green and violet as the three elementary color-sensations, was not, as Helmholtz states, guided in their choice "by the consideration that the extreme colors of the spectrum occupied the privileged positions," but selected those colors on hearing of Wollaston's supposed complete analysis of the sun's light into red, greenish blue and violet colors, separated from each other in the spectrum by dark spaces.

We hear no more from Young about his theory of colors until 1807, when he published the first volume of his celebrated work, "A Course of Lectures on Natural Philosophy and the Mechanical Arts."* On page 439 *et seq.* of this work Young gives a concise statement of his views on the analysis of the sensations of color, and supports these views with conclusive experiments with rotating colored discs; but, strange to

* "We have heard it remarked," says Dean Peacock in his Life of Young, "that no writer, on any branch of science which the lectures treat of, can safely neglect to consult them, so rich is the mine of knowledge which they contain; and it is a well-known fact, that many important propositions and discoveries have been more or less clearly indicated in them, which have only been recognized or pointed out when other philosophers discovered them independently, or announced them as their own."

say, he omits from this account of his theory all mention of the physiological explanation of it which he gave in the Bakerian Lecture of 1801. The following extracts from the Natural Philosophy give all that it contains on the theory of colors. The italics are our own.

"It has generally been supposed, since the time of Newton, that when the rays of light are separated as completely as possible by means of refraction, they exhibit seven varieties of colour, related to each other with respect to the extent that they occupy, in ratios nearly analagous to those of the ascending scale of the minor mode in music. The observations were, however, imperfect, and the analogy was wholly imaginary. Dr. Wollaston has determined the division of the colored image or spectrum in a much more accurate manner than had been done before: by looking through a prism, at a narrow line of light, he produces a more effectual separation of the colors than can be obtained by the common method of throwing the sun's image on a wall. The spectrum formed in this manner *consists of four colors only, red, green, blue and violet*, which occupy spaces in the proportion of 16, 23, 36 and 25, respectively, making together 100 for the whole length; the red being nearly one-sixth, the green and the violet each about one-fourth, and the blue more than one-third of the length. *The colors differ scarcely at all in quality within their respective limits, but they vary in brightness;* the greatest intensity of light being in that part of the green which is nearest the red. *A narrow line of yellow is generally visible at the limit of the red and green*, but its breadth scarcely exceeds that of the aperture by which the light is admitted, and *Dr. Wollaston attributes it to the mixture of the red with the green light.* There are also several dark lines crossing the spectrum within the blue portion and in its neighborhood, in which the continuity of the light seems to be interrupted. This distribution of the spectrum Dr. Wollaston has found to be the same, whatever refracting substance may have been employed for its formation; and he attributes the difference, which has sometimes been observed in the proportions, to accidental variations of the obliquity of the rays. The angular extent of the spectrum formed by a prism of crown glass is $\frac{1}{37}$ of the deviation of the red rays; by a prism of flint glass $\frac{1}{70}$." Fig. 3.*

Fig. 3. "The spectrum produced by looking through a prism at a narrow line of light.

* These figures, 3 and 4, are copied of the exact size of those given by Young in the plates appended to his Natural Philosophy. The descriptions under the figures are those given by Young. The colors in Young's figures we have indicated in type. There are six other figures illustrating Young's account, but we have found it impossible to convey *in print* clear ideas of their tints.

3.

"A narrow line of Yellow."

"In light produced by the combustion of terrestrial substances, the spectrum is still more interrupted; thus, the bluish light of the lower part of the flame of a candle is separated by refraction into five parcels of various colours; the light of burning spirits, which appears perfectly blue, is chiefly composed of green and violet rays: and the light of a candle into which salt is thrown abounds with a *pure yellow, inclining to green*, but not separable by refraction. The electric spark furnishes also a light which is differently divided in different circumstances.

"If the breadth of the aperture viewed through a prism is somewhat increased, the space occupied by each variety of light in the spectrum is augmented in the same proportion, and each portion encroaches on the neighboring colours, and is mixed with them; so that the red is succeeded by orange, yellow, and yellowish green, and the blue is mixed on the one side with the green, and on the other with the violet; and it is in this state that the prismatic spectrum is commonly exhibited.

4.

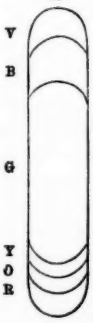


Fig. 4. "The appearance of a circular aperture, moderately large, viewed through a prism.

* * * * * * *

"Sir Isaac Newton observed that the effect of white light on the sense of sight might be imitated by a mixture of colours taken from different parts of the spectrum, notwithstanding the omission of some of the rays naturally belonging to white light. Thus, if we intercept one-half of each of the four principal portions into which the spectrum is divided, the remaining halves will still preserve, when mixed together, the appearance of whiteness; so that it is probable that the different parts of those portions of the spectrum which appear of one colour, have precisely the same effect on the eye. It is certain that the perfect sensations of yellow and of blue are produced respectively by mixtures of red and green, and of green and violet light, and there is reason to suspect that those sensations are always compounded of the separate sensations combined; at least, this supposition simplifies the theory of colours; it may, therefore, be adopted with advantage, until it be found inconsistent with any of the phenomena; and we may consider white light as composed of a mixture of red, green and violet, only, in the proportion of about two parts red, four green, and one violet, with respect to the quantity or intensity of the sensations produced.

"If we mix together, in proper proportions, any substances exhibiting these colours in their greatest purity, and place the mixture in a light sufficiently strong, we obtain the appearance of perfect whiteness; but in a fainter light the mixture is grey, or of that hue which arises from a combination of white and black; black bodies being such as reflect white light, but in a very scanty proportion. For the same reason, green and red substances mixed together usually make rather a brown than a yellow colour, and many yellow colours, when laid on very thickly, or mixed with black, become brown. The sensations of various kinds of light may also be combined in a still more satisfactory manner by painting the surface of a circle with different colours, in any way that may be desired, and causing it to revolve with such rapidity, that the whole may assume the appearance of a single tint, or a combination of tints resulting from the mixture of the colours.

"From three simple sensations, with their combinations, we obtain seven primitive distinctions of colours; but the different proportions in which they may be combined afford a variety of tints beyond all calculation. The three simple sensations being red, green and violet, the three binary combinations are yellow, consisting of red and green; crimson, of red and violet; and blue, of green and violet; and the seventh in order is white light, composed of all three united. But the blue thus produced, by combining the whole of the green and violet rays, is not the blue of the spectrum, for four parts of green and one of violet make a blue differing very little from green; while the blue of the spectrum appears to contain as much violet as green; and it is for this reason that red and blue usually make a purple, deriving its hue from the predominance of violet.

"It would be possible to exhibit at once to the eye the combinations of any three colours in all imaginable varieties. Two of them might be laid down on a revolving surface, in the form of triangles placed in opposite directions, and the third on projections perpendicular to the surface, which, while the eye remained at rest in any one point, obliquely situated, would exhibit more or less of their painted sides, as they passed through their different angular positions: and the only further alteration that could be produced in any of the tints would be derived from the different degrees of light only. The same effect may also be exhibited by mixing the colours in different proportions, by means of the pencil, beginning from three equidistant points as the centers of the respective colours."

On certain portions of the above extracts, which I have italicized, I will venture a few observations. In the first passage, thus indicated, Young says: "The *spectrum formed in this manner consists of four colours only, red, green, blue and violet.*"

Young then replaces Wollaston's "yellowish green" by "*green*," and farther on he adds, "The colours differ scarcely at all in quality within their respective limits, but they vary in brightness; the greatest intensity of light being in that part of the green which is nearest to the red," thus confirming our view that both Young and Wollaston were of the opinion that when a narrow bright crevice is observed through a prism, that the spectrum so viewed consists of only four colors, red, green, blue and violet, "differing scarcely at all in quality within their respective limits;" which limits they supposed naturally existed in the dark spaces which, as they imagined, bounded these elementary colors. Young, however, somewhat modifies this opinion in the next sentence, when he says: "A narrow line of yellow is generally visible at the limit of the red and green, but its breadth scarcely exceeds that of the aperture by which the light is admitted, and Dr. Wollaston attributes it to the mixture of the red with the green light." It would, indeed, appear from the last portion of this sentence that Young obtained directly from Wollaston one of the main facts on which his theory was founded, namely, that yellow can be reproduced by the mixture of red and yellow lights. But Wollaston, in his paper of 1802, from which we have cited, makes no such statement as to the composition of yellow light, and it is therefore probable that Wollaston communicated orally this view of the subject to Young. Every student of optics now knows that the description, already given, of Fraunhofer's observation on the color composition of the spectrum is the correct one; yet the errors of observation of Wollaston and of Young were errors which led to a great discovery, as we shall see on the further examination of the history of this beautiful and comprehensive theory of color.

Farther on in the Natural Philosophy we read that, "The sensations of various kinds of light may also be combined in a still more satisfactory manner by painting the surface of a circle with different colours, in any way that may be desired, and causing it to revolve with such rapidity, that the whole may assume the appearance of a single tint, or of a combination of tints, resulting from the mixture of the colours." These experiments were evidently first made by Young; and are fully described in the text and perfectly illustrated in the colored discs in the plates of Young's work. These experiments have been carefully repeated by Helmholtz, Maxwell and others, and of their general accuracy there is no doubt. We can readily imagine the delight with which Young must have viewed these beautiful experiments, which, however, together with other truths unfolded by him, were destined to remain unnoticed, "until a later generation, by slow degrees, arrived at the *discovery* of his discovery."

It must now recur to the reader to inquire, *when* were made these experiments which first confirmed Young's hypothesis and placed it among the best established truths of optical science; and why it was that Young should for so long a time have been satisfied with a hypothetical statement of his views on the color-sensations, and should have deferred to bring those views to the test of experiment. For reasons already stated, Young, in July, 1802, changed his three elementary color-sensations, red, yellow and blue, to red, green and violet. The experiments with the rotating colored discs were first published in 1807. Young printed the syllabus of his first course of lectures on January 19th, 1802, in a volume of 250 pages. I have not been able to procure a copy of this syllabus, but evidently it does not contain even the corrected statement of his theory of color, for that was based on Wollaston's observation, which appeared subsequently to the syllabus, on June 24th, 1802. It is therefore evident that unless Young made the experiments with the rotating colored discs during the latter part of his course of lectures, he must have made them during the interval between his retirement from the professorship at the Royal Institution and the publication of his lectures on Natural Philosophy in 1807. Young delivered his first lecture before the Royal Institution on January 20th, 1802, and was very busy with his lectures until July 4th, 1803, when he retired. I think that we may fix the date of these remarkable experiments as somewhere between 1803 and 1807, and it is highly probable that the theory was never given to the public in a lecture before the Royal Institution, but first appeared in the publication of his Lectures on Natural Philosophy.

That Young should have delayed to bring to the test of experiment a plausible hypothesis, when other men would at once have appealed to the instruments in their laboratories, is explained by the fact that Young "at no period of his life was fond of repeating experiments or even of originating new ones. He considered that, however necessary to the advancement of science, they demanded a great sacrifice of time; and that, when a fact was once established, that time was better employed in considering the purposes to which it might be applied or the principles which it might tend to elucidate." Indeed, this peculiarity receives abundant confirmation from his own words; thus, in the Bakerian Lecture, already so often referred to, he says, "Nor is it absolutely necessary in this instance (in speaking of the proofs to be adduced in support of the undulatory theory of light) to produce a single new experiment; for of experiments there is already an ample store;" and in a letter written in November, 1827, to his sister-in-law, Mrs. Earle, on the respective honors given by Herschel, in his Optics, to Young

and Fresnel, he says: "And acute suggestion was then, and indeed always, more in the line of my ambition than experimental illustration." Young carried his opinion of the secondary importance of experiment so far as even to object to the increase of the fund left by Wollaston to the Royal Society to aid experimental inquiries, in these words: "For my part, it is my pride and pleasure, as far as I am able, to supersede the necessity of experiments, and more especially of expensive ones."

ART. XXVIII.—*A redetermination of the Constants of the Law connecting the Pitch of a Sound with the Duration of its Residual Sensation;* by ALFRED M. MAYER.

IN my "Researches in Acoustics, Paper No. 6," published in this Journal in October, 1874, I gave the result of many experiments on the durations of the residual sonorous sensations, and embodied those determinations in this law:

$$D = \left(\frac{53248}{N+23} + 24\right) \cdot 0001,$$

in which D = the duration of the residual sonorous sensation corresponding to N number of vibrations per second.

The precise determination of the durations of the residual sonorous sensations are difficult by reason of the complex character of the sound perceived when the vibrations of a tuning fork are sent intermittently into a resonator by means of a revolving perforated disc; and the difficulty of the determination is increased by the fatigue and deadening of the sensitiveness of the ear produced by the beats which enter it from the resonator.

The important applications that have been made of this law in the physiology of audition, and in the elucidation of the fundamental laws of musical harmony, have made me desire to have my determinations reviewed by ears more highly cultivated than mine in the appreciation of pitch and of musical intervals, and more skilled in the direct analysis of composite sounds into their simple component tones. Since my publication in October last, I have had the good fortune to have elicited in Madame Emma Seiler and in her son, Dr. Carl Seiler, a profound interest in my researches. They have spent considerable time in the redetermination of the durations of the residual sonorous sensations; making use, under my directions, of the same apparatus which I employed in my original experiments. Madame Seiler assisted Helmholtz in the experiments contained in his renowned work on physiological acoustics, and unites to educated

musical perceptions a thorough knowledge and appreciation of all recent advances in physiological acoustics. I have, therefore, great confidence in the following results, which I desire my

S	N	D	L
UT_1	64	$\frac{1}{256}$ = ·0395 sec.	2·5
UT^2	118	$\frac{1}{45}$ = ·0222 "	2·8
UT_3	256	$\frac{1}{70}$ = ·0142 "	3·6
SOL_3	384	$\frac{1}{102}$ = ·0098 "	3·7
UT_4	512	$\frac{1}{130}$ = ·0076 "	3·9
MI_4	640	$\frac{1}{153}$ = ·0065 "	4·1
SOL_4	768	$\frac{1}{166}$ = ·0060 "	4·6
UT_5	1024	$\frac{1}{180}$ = ·0055 "	5·6

readers to substitute for those contained in the table given on page 244 of vol. viii of this Journal.

From the above data the law given on page 146 becomes

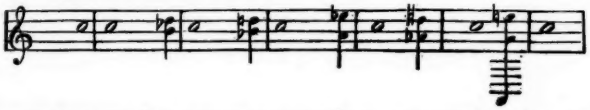
$$D=\frac{3\cdot2}{N+31}+\cdot0022.$$

The adoption of the law with these new constants requires the following corrections to be applied to my paper:

Page 246, *dele* "The ordinate of MI_4" &c., to end of paragraph.
" 249, line 19 from bottom, for $\frac{1}{11}$ read $\frac{1}{256}$.
" " " 17 " " " " $\frac{1}{356}$ " $\frac{1}{4356}$.
" 250, " 1 " top, " $\frac{1}{16}$ " $\frac{1}{15}$.
" " " 3 " " " " " "
" " " 6 " bottom, " $\frac{1}{3297}$ " $\frac{1}{3291}$.
" " " 5 " " " $\frac{1}{259}$ " $\frac{1}{253}$.
" " " 3 " " " $\frac{1}{256}$ " $\frac{1}{1256}$.

The corrections under "5. *Quantitative applications of the Laws to the fundamental facts of Musical Harmony,*" can be readily applied from the law as given above. Suffice it here to say that on the table on page 252 the nearest consonant interval in the octave of C_1 is a Fourth+$\frac{2}{3}$ of a semitone; while in the octave of C_6 the nearest consonant interval of two simple sounds has contracted to only one tone.

In the opening part of this communication I spoke of the difficulty of the determination of the residual sonorous sensations by reason of the complex character of the sounds perceived when the vibrations of a tuning fork are sent intermittently into a resonator by means of a revolving perforated disc. I will now describe the character of the successive sensations experienced when, starting from rest, we gradually increase the velocity of rotation of the disc until the separate beats blend into a smooth continuous sensation. When the disc is stationary, with one of its openings opposite the mouth of the resonator, it is evident that the ear will experience a simple sonorous sensation when

a tuning fork is brought near the mouth of the resonator; on revolving the perforated disc, two additional, or secondary, sounds appear; one slightly above, the other slightly below the pitch of the fork. An increased velocity of rotation causes the two secondary sounds to diverge yet farther from the note of the beating fork, until the velocity reached is so great that the two secondary sounds become separated from each other by a major sixth, while, at the same moment, a resultant sound appears, formed by the union of the sound of the fork with the upper and the lower of the secondary sounds. This resultant is the second octave below the note given by the fork. On further increasing the velocity of the disc, the two secondary sounds and the resultant disappear, and the ear has alone the sensation of the simple sound produced by the beats of the fork; which, at this stage of the experiment, blend into a smooth continuous sensation. These successive and gradual changes, as they happen with a UT_4 fork, we have indicated in steps of semi-tones in the appended musical notation. The sound of the fork is given in the semibreve; the crochets represent the secondary sounds and the resultant sound. In the fourth bar the upper note E flat proceeds to D sharp in the fifth bar. This is so because, in the natural scale, D sharp is higher in pitch than E flat.

In conclusion, I request my readers to transfer the comma from after *Dissonanz* to after *continuirliche*, in the quotation from Helmholtz at the beginning of my paper (vol. viii, p. 241).

ART. XXIX.—*On the Action of the Less Refrangible Rays of Light on Silver Iodide and Bromide;* by M. CAREY LEA, Philadelphia.

IT will be the object of the present investigation to show:

1st. That silver iodide and bromide are sensitive to all the colored rays of the spectrum.

2d. That silver iodide is to all the less refrangible rays more sensitive than silver bromide.

3d. That the theory of M. E. Becquerel as to existence of "exciting rays" and "continuing rays" is not supported by a careful examination of the phenomena in question.

The first of these positions differs from those generally accepted in extending farther to the less refrangible end of the

spectrum the sensitiveness of both the compounds in question, especially of silver iodide.

The second of these positions, that in reference to the comparative sensitiveness of AgI and AgBr, differs essentially from the views hitherto accepted, according to which AgBr has been held to be by far the more sensitive to the less refrangible rays. I shall endeavor to show that the contrary is the case.

In these investigations I have confined myself to studying the effects obtained upon the silver compounds as formed in the body of pure paper, applying in all cases the silver solution after that of the alkaline haloid, and immediately washing out the excess of silver nitrate.

The colored light was separated by means of colored glasses. Without wishing to detract from the value of observations made with the prismatic spectrum, I am inclined to believe that those obtained with colored glasses, whose absorption spectra have been carefully and exactly made out with the aid of spectroscopic analysis, are at least equally reliable, and in some cases, certainly more decisive. It is also to be remarked that prismatic solar spectra, as usually obtained, are open to the suspicion of not being absolutely pure, but liable to admixture of rays coming from light falling upon the prism outside of the true image of the slit, by reason of diffraction, and also possibly from other sources. A very careful prismatic analysis of the spectrum itself would be necessary to detect the presence of faint violet light cast by any such means on the red end of the spectrum. With colored glass, on the contrary, the detection of any such admixed color may be accomplished by a simple inspection of its absorption spectrum. If it be suspected, for example, that a given piece of red glass, or any number of pieces, permit the passage of a trace of violet light, the spectroscope gives us at once a decisive answer, because we can project any such violet light that may be present against an absolutely dark field of view, and decide positively on its presence or absence. Another serious difficulty with the spectrum arises from the very long exposure which is necessary to make apparent the effect of the less refrangible rays, and which long exposure can be less advantageously managed with the solar spectrum than with colored glass.

Still another advantage in the use of colored glass is that the pieces used can be of any desired size, so that one can operate over a large space and can simultaneously expose several preparations to identical influences, and thus obtain a more accurate comparison than is possible with the successive exposures necessary with the solar spectrum. By interposing a suitable glass negative between the colored glasses and the paper, the effects of the exposure are rendered much more marked: the distinctness of the reproduction of the image of the nega-

tive serves in each case as a measure of the degree of sensitiveness. This has a particular importance when a development process is used, because even with the utmost care, there will sometimes be a discoloration arising from the action of the development bath. And if the arrangements are such that the action of light produces a flat tint only, it may easily become impossible to distinguish between the effects due to the development of an impression of light and those depending upon a spontaneous deposit from the bath; moreover, this spontaneous deposit is most apt to occur, in the case of a very weak impression, exactly where it is most able to induce error. The employment of a negative avoids this difficulty, and if its image has a great variety of tones, the observer is able to measure very closely the relative extent to which it is reproduced on the sensitive surface.

Direct or Developed Images.—A certain amount of ambiguity has been introduced into what has been published on the subject of the sensitiveness of silver salts, by not distinguishing in all cases whether the sensitiveness spoken of relates to images which appear during the exposure, or are subsequently evoked by development. The capacity to form developable images is in all cases the true test of sensitiveness: any other criterion would lead to most erroneous conclusions. In this paper it will always be understood, except when otherwise mentioned, that the sensitiveness referred to is that which is exhibited under a development by means of gallic acid and silver nitrate, controlled by acetic acid.

The papers used were first floated on weak solutions of potassium iodide and bromide respectively, using them in equivalent proportions, so that each might take up an equal quantity of silver from the silver bath. This last was always made of the same strength, viz: 30 grains to the ounce, and a fresh solution was used for every piece, because, as silver iodide and bromide are soluble in solutions of silver nitrate, there might otherwise result a transfer and the formation of appreciable quantities of silver iodobromide, the reactions of which substance are different, and so energetic that the presence of the least trace would destroy all value in the results obtained. For the same reason the potassium salts employed must be pure. Therefore the KI and KBr, which were exclusively used, were submitted to careful examination at the outset. The results subsequently obtained as to sensitiveness of AgI were so different from what has hitherto found acceptance, that it was deemed advisable at the close to submit the KI to a second rigorous examination for bromine. This examination fully confirmed the first conclusions as to the absolute purity of the specimen used.

The papers were next blotted off in clean filtering paper (because if hung up to dry, the lower end becomes more richly charged than the upper), and after complete removal of the surface moisture were floated on the silver nitrate solution. They were next thrown into a large vessel of water, washed with running water and dried. In a few cases specially mentioned, the silver solution was allowed to dry on the paper for comparative experiment.

Red Rays.

Examination of the Glass.—The ruby glass of commerce differs very much in different specimens; some pieces, though appearing to the eye to be of a strong, pure red, nevertheless transmit a good deal of green light, and show in the spectroscope a strong green band. Other pieces are of a much purer color, but I have never met with any, even the darkest, which did not, with a critical examination, show in their absorption spectrum traces of the more refrangible rays. The band which represents the proper color extends from very near the extreme red end to a point a little beyond the double sodium line D.

When two such pieces are superposed, the trace of more refrangible light is excluded. The proper band of color is also shortened at its more refrangible end, and recedes to the other side of D. Two pieces, a dark red and a medium red, gave as their limit a wave-length λ 594. Two dark pieces, superposed, had a limit of λ 600. A large number of experiments were made with the combination last mentioned. But, as it was my desire to make these investigations, as far as possible, absolutely correct, all my results were revised, and confirmed by final trials with light filtered through *three* thicknesses of ruby glass. These showed as the limiting wave-length of their absorption spectrum, λ 605, a limit which totally excludes the yellow, and represents the red rays with at most a slight admixture of orange, as the light at and near this limit of λ 605 was extremely feeble, and the whole strength of the band found its place beyond. The measures here given were repeated a number of times on different days, with concurrent results.

Result.—With *two red plates*, having a limit of λ 594, a strong, *direct* image was obtained upon the iodide paper by an exposure of 15 minutes to bright sunshine. The bromide paper, exposed under the same glasses and negative, simultaneously, showed nothing. By a very long development, exceedingly faint traces of an image were rendered visible. The contrast between these two was very striking.

With *two stronger red plates*, having a limit of λ 600, and of course a diminished illumination, no trace of an image by devel-

opment could be obtained on the silver bromide paper, after exposures of various lengths up to an hour and a half of brilliant sunshine (from 12.15 to 1.45 P. M., Jan. 30, 1875, bright sunshine on snow).

On the contrary, silver iodide gave, after 20′ exposure, a developed image showing some detail, and with 40′ exposure, a faint, *direct* image, visible without development.

With *three red plates*, having a limiting wave-length λ 605, there was necessarily a further great diminution of illumination. Nevertheless, silver iodide gave, with 30 minutes' exposure (middle of the day, bright sunshine on snow, Feb. 1, 1875), a faint, and with four hours' exposure, a full image. Feb. 2d, three hours' exposure gave an image showing considerable detail. Feb. 5th, same result. These were of course all developed images.

The corresponding bromide papers, receiving identical exposures under the same glasses, side by side with the iodide, absolutely failed to develop anything. These developments were prolonged for several hours, in order that the faintest traces, if present, might render themselves visible. But in no case did silver bromide, when exposed under the three red glasses, show the faintest trace of any image, even with a four hours' exposure. That silver bromide is not wholly destitute of sensibility to red rays, was shown by the previous experiments; but this sensibility is small, and when the red light is not only very pure, but very faint, it may be exerted for a very long time without result. Still, I believe that with a day's exposure under a strong summer sun, an impression capable of development could be produced. On the other hand, in no case, even with exposure as short as half an hour to a winter sun, did silver iodide fail to give an image, under these three ruby glasses, whose faintest light terminated at λ 605. It appears, therefore, certain that both silver iodide and silver bromide prepared on paper, with excess of silver nitrate removed, are distinctly sensitive to red light, and that silver iodide is, under these conditions, at least ten times as sensitive as silver bromide.

Yellow Rays.

It is by far more difficult to isolate yellow rays than either red or green, because almost all media that transmit yellow rays also transmit red, and many also transmit green. The yellow glass that is found in commerce lets through the whole spectrum, except the extreme violet end, and for a time I thought that the isolation of the yellow rays by colored glass would be impracticable. I finally succeeded very well by combining a deep brown glass with a dark green. The brown glass transmitted the yellow and red, absorbing the rest, and

the dark green admitted principally green and yellow, cutting off the orange and red.

A spectroscopic analysis gave the following results:

Extreme limit of wave-length at less refrangible end of the spectrum,	**λ 638**
At more refrangible end,	**λ 527**
Point of maximum illumination,	**λ 570**

The limits here given are extreme limits at which the absorption spectrum ended. It was estimated that *at least* nineteen-twentieths of the illumination was pure yellow, with perhaps a very faint admixture of orange, and of the less refrangible green rays, bordering on the yellow (any close observation of the spectrum will show how little pure yellow light it contains).

Result.—Silver iodide showed itself also more sensitive to yellow light than silver bromide, but the difference was greatly less than in the case of red light, and the results were more variable. Feb. 4, 1875, with exposure to bright sunlight from 12 M. to 3 P. M., distinct images were got by development on silver iodide, whilst with four trials of equal length on bromide paper, nothing was got. On the following day, with an exposure of about equal length, but before midday, well marked images were obtained on silver bromide. Those obtained at the same time on silver iodide, exposed side by side, developed to an equal strength in one-third the time.

I conclude, therefore, that silver iodide and bromide are both sensitive to yellow light, and the iodide more so than the bromide.

Green Rays.

Much of the green glass found in commerce admits nearly the whole spectrum except the red rays. There exists, however, a very dark shade of green, which narrows the transmitted band very much. When two such pieces of dark green were superposed, their absorption spectrum was as follows:

Extreme limit toward red end,	**λ 601**
Extreme limit toward blue end,	**λ 488**

This last limit corresponds very nearly with the solar line F, whose wave-length is 486, and which approximately separates the green from the less refrangible blue. For exact experiments, however, a narrower band is desirable. Accordingly, three plates were superposed. This involved such a reduction of illumination that the sun could be viewed through the glass without inconvenience. This combination gave the following measurement:

Extreme limit toward red end,	**λ 581**
Extreme limit toward blue end,	**λ 497**

Near these limits the light was extremely faint. To ascertain the rays of really effective strength transmitted by this combination of plates, another measurement was taken, resulting as follows:

Effective limit toward red end,	**λ 569**
Effective limit toward blue end,	**λ 517**

The portions of the band which lay between λ 569 and λ 581 on the one side, and λ 497 and λ 517 on the other, were so extremely faint as to have no appreciable agency in affecting the result.

It will be observed that even the limit of the faintest rays does not extend as far as the solar line F; the blue is therefore absolutely excluded. Toward the yellow side, even the faintest rays do not extend so far as the sodium double line D and the effective rays terminated at λ 569. The yellow is therefore virtually excluded.

Result.—With two dark green glasses (λ 488–601). A powerful image was developed on silver iodide after three minutes' exposure. On silver bromide the same exposure gave faint traces of an image only. The exposure of an hour and a half failed to produce on silver bromide as strong an impression as did the three minutes on the iodide.

An exposure of forty minutes gave on silver iodide a plain, direct image. With ninety minutes, no direct image was produced on silver bromide. Other exposures and degrees of exposure gave concurrent results.

With three dark green glasses (extreme limit λ 497–581, effective λ 517–569). Silver iodide gave the following results: Two and a half minutes, faint image developed. Six minutes, distinct; fifteen minutes, strong; thirty, very full exposure; three hours and a half, completely overdone.

Silver bromide: two and a half, and six minutes, nothing; fifteen, faint trace; thirty, a little stronger; three and a half hours, moderately strong.

Comparing the above: AgBr with fifteen minutes about the same as AgI with two and a half; AgBr with thirty minutes not quite so strong as AgI with six: AgBr with three and a half hours much the same as AgI with fifteen minutes, if anything less strong. Other trials gave conformable results.

The conclusion seems, therefore, irresistible that AgI and AgBr both are sensitive to pure red, yellow and green rays; and that AgI, at least under the conditions of these experiments, show to yellow rays a slightly superior, and to green and red rays a markedly superior sensitiveness.

AgI and AgBr together.—Although the scope of this investigation was directed to the action of pure AgI and pure AgBr, a few

experiments were made on the two used together. They were used in equivalent proportions and in such quantity that the paper containing the two salts together should take up exactly as much silver as the papers prepared with AgBr and AgI separately. The result demonstrated a materially greater sensitiveness to both red and green light than with either used separately. The experiment was not extended to the yellow, but it would doubtless have given similar results, as also the more refrangible rays of the spectrum.

[It may be worth mentioning, in passing, that the result of this part of the investigation led to the making of a series of experiments on the introduction of silver iodide into the "emulsion process" for preparing photographic dry plates. The result was that when either equal or equivalent quantities of AgBr and AgI were employed, nothing material was gained, but when the quantity of AgI was much reduced, and especially when AgCl was added, plates were obtained possessing a degree of sensitiveness much beyond anything heretofore attained by any collodion method, either wet or dry. A fully exposed image was obtained by an exposure under a negative to a gas flame for one second. The details necessary to the production of this result have been communicated to the Photographic Society of Philadelphia.]

Sensitivensss to White Light.—Although this investigation was directed to the action of certain colored rays, a few experiments were included upon the comparative action of white light on AgI and AgBr. There was found a moderate superiority of sensitiveness in AgI, though much less marked than in the case of the red and green rays. Exposure under a negative for three seconds to a weak diffused light sufficed to produce a strong latent image on both the AgI and AgBr papers, but most strong upon AgI.

Influence of Free Silver Nitrate.—The presence of free silver nitrate in a film of AgBr considerably increased the capacity to receive a latent image, but its agency in producing a direct image was still more marked. Thus an exposure of ninety minutes under two green glasses (limits λ 488–601) failed to produce any visible image on washed AgBr, whereas when the paper contained free silver nitrate, a plainly visible direct image was obtained in forty minutes.

As respects silver iodide, my opinions have always differed from those which have prevailed among photo-chemists. Many years ago I proved the opinion that silver iodide, absolutely isolated, was insensitive to light, to be erroneous. I covered glass plates (preferably ground glass, for better adhesion) with thin specular films of silver, and then iodized these through and through by means of a solution of iodine, and succeeded

without difficulty in developing images received on such films.

The opinion also that silver iodide, in the absence of free silver nitrate, is comparatively insensitive, receives its disproof from the foregoing investigation, which decisively shows that washed films of silver iodide possess a high degree of sensitiveness to white light, and some sensitiveness to the less refrangible rays: in either case a higher degree than silver bromide.

Exciting and Continuing Rays.

More than thirty years ago, M. E. Becquerel published his well-known theory of the existence of two classes of rays, the "exciting" and the "continuing" rays. According to it, the more refrangible rays had alone the power of originating an impression; the less refrangible were powerless to commence, but capable of continuing and re-inforcing an impression which had been commenced by the more refrangible rays. This view is still maintained by M. Becquerel in his comprehensive work "La Lumière," published a few years since, and is adopted by Jamin, Ganot and others in their treatises on physics, though the acceptance has been far from universal.

The foregoing results are evidently quite incompatible with this theory, for they prove the existence in the less refrangible rays of the power to impress both silver iodide and bromide. In only one way could this testimony be set aside, namely, by the allegation that these papers might have received, in the course of their preparation, some chance effect of light sufficient to lay the foundation of an impression, and that this was afterward continued only, not originated, by the colored light to which the papers were subsequently exposed.

It would seem to be a sufficient answer, that all due care was taken to avoid this danger, and, as the experiments themselves showed, successfully. Such an uncertainty might exist with experiments made with the spectrum, but not where, as in these investigations, a negative is interposed. For, according to Mr. Becquerel, the preliminary impression of light which is necessary in order to enable the less refrangible rays subsequently to exert their continuing power on AgBr, AgI or AgCl formed on paper, is that which would enable them to receive a development by gallic acid, etc. When this impression has been received the less refrangible rays can continue this commencement, and this result may be obtained either with the spectrum, or with colored glass.*

But as in my experiments the latent images were always developed with gallic acid, it follows that, had the images evoked

* La Lumière, vol. ii p 76.

been due to a continuing power of the colored light upon an original impression caused by light accidentally admitted during the preparation, then the papers must have darkened all over by the agency of gallic acid, instead of developing an image. So that the system of experiment adopted was in itself incidentally a precaution against any such source of error.

Nevertheless, wishing to obtain a result that would be entirely decisive, paper was prepared for this special purpose under conditions of exceptional care. It was prepared at night, using light, not only most carefully guarded by colored glass, but as little of it as was possible to manipulate by. Even this faint light was carefully excluded from the preparations. The paper was covered whilst being sensitized on the silver bath and was then washed with special precaution and dried in absolute darkness. As soon as dry it was placed in the frames and under the glasses under which it was to be exposed next day. The precautions taken enable me to say that most certainly the paper received in its preparation no effect of light capable of influencing a development in any way. Its action proved to be precisely the same as that of the paper prepared in my usual way, thus demonstrating that all the rays of the spectrum possess the power of originating an impression on silver iodide and bromide.

In the foregoing pages, I have briefly given the result of one hundred and sixty experiments. The results, with such slight and altogether unimportant variations as necessarily arise from slight differences of preparation and differences in the character of the sun's light, were remarkably concordant, and may be summed up as follows:

1. AgBr and AgI are sensitive to all the visible rays of the spectrum.
2. AgI is more sensitive than AgBr to all the less refrangible rays and also to white light.
3. The sensitiveness of AgBr to the green rays was materially increased by the presence of free silver nitrate.
4. AgBr and AgI together are more sensitive to both the green and the red rays (and probably to all rays) than either AgI or AgBr separately.
5. There do not exist any rays with a special exciting or a special continuing power, but all the colored rays are capable both of commencing and continuing the impression on silver iodide and bromide.

Philadelphia, March 6, 1875.

Art. XXX.—*On the Silurian age of the Southern Appalachians;* by Frank H. Bradley.

1. *Introduction.*

Emmons, in his American Geology, refers most of the rocks along the western line of North Carolina* to his Taconic system, and says (vol. i, pt. 2, p. 24) that "the locality at the Warm Springs [Madison County, N. C.] is a good exhibition of the development of the Lower Taconic rocks in the Southern States;" but he also states (p. 25) that, in Cherokee County [near Murphy], the system is separated [divided] a few miles, by the interposition of a ridge of primary schists with staurolite.

Safford, in his Geology of Tennessee, 1869 (pp. 177–8), says, of the metamorphic rocks of the eastern border of that State: "A portion of the beds are certainly referable to the Ocoee group: the remainder, although conformable, may be older, and most likely are. * * The question of the greater age of [these] other parts is not so easily settled, and must remain open for the present. I know of no sufficient reason for referring any of these rocks to the Huronian or Laurentian series of Canada." Again (p. 193): After the Ocoee, the Chilhowee sandstones reappear and continue up the [French Broad] river to the State line. * * In North Carolina, a short distance beyond the line, the Ocoee group sets in again, and is the formation to within a mile of the Warm Springs. Then follows a Knox belt. The Springs are located on the Knox dolomyte. * * Beyond the Springs follow Chilhowee sandstones, then Ocoee rocks, and finally gneiss." The Knox group, here referred to, includes the Quebec group and the Calciferous: the Chilhowee sandstone is the typical Potsdam; and the Ocoee is the Lower Potsdam or Acadian group.

Professor Kerr has just issued a new geological map of North Carolina, which shows a strip of "Huronian" along the western line of the State, apparently covering the whole area there assigned by Emmons to the Taconic, even including the Warm Springs region. As the text of his report has not yet appeared,

* Since taking residence in Knoxville, in 1869, the writer has had his attention particularly drawn to the local features of the Lower Silurian rocks of East Tennessee and of the metamorphic strata which occupy the adjoining portions of North Carolina and Georgia. Most of the data given in this paper have been gathered incidentally, in connection with trips for the examination of mining properties; though, the clew to the Blue Ridge puzzle having thus been secured, a final trip was made solely for the purpose of completing its solution. Thus much to explain what some will regard as a trespass upon the preserves of the State geologists of North Carolina and Georgia, though neither of these officers has yet been able to make other than very hasty visits to the region in question. Without an opportunity of examining all the literature of the subject, reference can be made only to the works of Emmons, Safford and Kerr.

we cannot fairly criticize his non-adoption of the conclusions of Safford, although that geologist had followed the strata so connectedly, from their less altered and more fossiliferous regions, and was therefore least of all liable to be in error.* East of this Huronian is marked a belt of "Laurentian," the southern part of which is included in the area in question. The writer would here express an opinion, long held as a suspicion, but latterly confirmed by the results of the recent examinations of the crystalline rocks by Dana, Irving, Brooks and others, that the typical Huronian strata of the lake region, as well as large areas elsewhere so named, are simply metamorphic Silurian. It is pleasant to know, from correspondence, that some eminent geologists have reached the same conclusion.

The writer's conclusion, briefly stated, is that the rocks of that portion of North Carolina south and west of the Little Tennessee, together with the metamorphic area of Georgia, north of a line parallel with and ten miles south of the Chattahoochee (and *probably* that south of this line), and the entire metamorphic area of Alabama, are *Silurian or newer*, with the possible exception of two or three small patches not over ten miles in diameter. To the northeast of the Little Tennessee, there are large areas of rocks of the same age, but of undetermined boundaries, and including some patches which are pretty certainly Archæan. One of these latter forms and surrounds the "Bluff," on the borders of Cocke County, Tenn., and Madison County, N. C., where there are extensive outcrops of protogine and unakyte, with heavy beds of porphyritic hematite. This area—probably of the same age as the iron-bearing rocks of Missouri—adjoins the Warm Springs region before mentioned; and it may be well to say here that in Emmons' figure (l. c.) of the section there exposed, No. 15 is Chilhowee sandstone; 14 and 13 are Ocoee, separated by a fault from 12, which is Knox dolomyte, and whose cavernous structure give vent to the waters warmed by flowing through the deep fissures of the fault to the heated substrata; No. 11 is Knox shale; 10 is Chilhowee sandstone; 9 to 1 are Ocoee, probably resting upon Archæan. If this is "typical Lower Taconic," it is typical Lower Silurian as well. It is perhaps pertinent to note here that Emmons's Upper Taconic (l. c., pp. 62–68) is plainly only the slightly (or not at all) metamorphosed Silurian of the Great Valley.

* It is but justice to Professor Safford to say here that, during five years of frequent reference to his Report, in connection with field-work upon nearly all parts of the geological series exposed in East Tennessee, the writer, while recognizing many *local* deficiencies and some errors, such as are unavoidable in rapid work, has been constantly gratified and surprised at the thoroughness of the Report in all its *general* features. It has been and continues to be a great loss to Tennessee, as well as to science, that Professor Safford has not been employed to complete the detailed survey of the State.

Before giving further details of observations, it may be well to mention briefly the characters of the unaltered portion of at least the lower part of the series supposed to be represented by the metamorphic strata in question, beginning with the upper layers.

2. *Silurian of East Tennessee.*

Throughout East Tennessee the Cincinnati group is composed chiefly of shales, more or less calcareous, but includes generally two marked beds of limestone—the upper a red, compact, crinoidal marble; the lower, called by Safford the "iron-limestone," a strongly ferruginous rock, and so siliceous as to leave in many cases a tolerably solid skeleton, after its lime has all been removed by percolating waters, though in others, again, it decays entirely to a sandy clay. This lower bed is quarried, near Knoxville, as a flagging-stone, and proves very satisfactory, though its surfaces are quite irregular. Fine specimens of ripple-marks are common on these slabs; and the false-bedding of beach-sands is often well shown upon weathered edges of layers. The lowest beds of this group are generally shales. The total thickness has been estimated by Safford at about 1,850 feet.

The Trenton group is represented by heavy-bedded, red, gray and variegated marbles, of an estimated thickness of 380 feet, certain layers of which are extensively quarried and shipped for both building and monumental purposes.

The Chazy—"Maclurea beds" of Safford—consists of about 500 feet of bluish, impure limestone, generally very shaly, though at some points quite solid. The usual *Maclurea magna* is quite abundant, as are also various undescribed species of Sponges.

This is followed, below, by the Knox group, consisting of three members—a limestone, a shale and a sandstone. The limestone is mainly a very impure dolomyte, though with some purer layers, and includes also two or more thin beds of sandstone, one of which is probably the equivalent of the Saccharoidal sandstone of Missouri. The middle and lower portions are especially siliceous, being commonly crowded with cherty nodules and bands, the layers sometimes showing a thickness of five feet or more of solid chert. These are so numerous that the outcrops of this series are constantly marked by vast quantities of cherty masses, large and small, and more or less disintegrated, according to the varying percentage of included dolomyte. This included mineral is often crystallized, and its removal by weathering leaves numerous rhombohedral cavities, which Safford has noted as peculiar to chert of this horizon. Perhaps, a still more constant mark is the oölitic structure, which

is often very perfect throughout large masses. This structure also frequently occurs in the lower layers of the limestone, where the upper graduates into the middle member, or Knox shale. This upper member carries a greater variety of ores than any other bed in all this region; but, as yet, it appears doubtful whether any of them are in sufficient quantity and of such purity as to be of much practical value. Pyrite, galenite and sphalerite are of frequent occurrence in small quantities; and, at a few points, there are indications of considerable bodies of lead and zinc ores, the weathered portions of the latter showing much calamine and smithsonite. Barite is also quite abundant, though often impure. Fluorite also occurs in small quantities. These mainly occur at the filling of cavern-like chambers irregularly following the "joints" of the strata; but little is yet known of their extent, and nothing as to any law of their distribution. They also occur in grains disseminated through the mass of the rock. The most abundant ore along the outcrop of this series is limonite, which occurs in immense beds, both compact and ochreous. It is plainly a surface accumulation, resulting from the decomposition of pyrite and siderite in the limestone, and varying greatly in degree of purity, according to local conditions. Some beds are so filled with larger or smaller masses of chert as to be worthless; while others are almost entirely free from such impurities. Most of this ore contains more or less manganese ore, partly in the form of wad, partly as psilomelane. The very general dissemination of iron through the limestone, as sulphide, as oxyd, or as carbonate, is evidenced by the color of the heavy beds of reddish-brown clay accompanying every outcrop, which have been formed from the impurities of the rock, as the lime and magnesia salts have been dissolved away.*

* These immense beds of debris, along the outcrops of all but the very hardest and most siliceous rocks, are the constant characteristic of all this southern region, and, with the entire absence of drifted material above the level of the old river-terraces, afford the best of evidence that no glaciers have ever existed in the region. The immense amounts of river gravel, however, which bestrew the slopes of the valleys, up to from 170 to 200 feet and more above the present stream-levels, would indicate vast accumulations of snow upon the higher parts of the mountains; and it is not improbable that traces of glacial action may yet be found about their summits. These deposits indicate that, about Knoxville, the Holston was 170 feet above its present level, and filled a valley at least four miles wide, while its discharge was by the valley of the Coosa River, directly to the Gulf of Mexico, instead of by the present valley of the Tennessee. The Clinch must then have formed the main source of the Tennessee, and possibly this may also have found a southwestern outlet after passing Chattanooga.

The deep disintegration of the rocks will also account for the *entire absence of lakes* in this region, since any body of water, if ever formed here, would find little difficulty in cutting down its outlet so as to drain its bed. The most southern body of water in the Appalachians is said to be at Mountain Lake, Giles County, West Virginia, which is not far from the region, on the Greenbrier River, where Stevens recently reported finding glacial markings. (This Journal, III, vi, 371).

The middle member of the Knox series is the Knox shale, which in the neighborhood of Knoxville is a nearly pure clay shale; but it is said to become more calcareous to the northeast, forming at some points a pretty solid limestone, not readily distinguished from the Knox dolomyte proper. The Knox sandstone is ferruginous and quite commonly very argillaceous and glauconitic. The Knox group, as a whole, represents the Quebec group and the Calciferous sandrock; and the plane of division is not readily determined. The thin bands of limestone at the top of the shale contain numerous trilobites, mostly of species as yet undescribed, which appear to be closely related to those occurring at the base of the Quebec group in Canada. The maximum thicknesses are stated by Safford as: Dolomyte, 4,000 feet; shale, 1,500 to 2,000 feet; sandstone, 800 to 1,000 feet. At some points, the two lower members are very thin or even wanting.

Next comes the Chilhowee sandstone, generally a heavy-bedded rock, occasionally white, generally ferruginous, often pyritous. This, at most of its outcrops, is metamorphosed into an extremely compact quartzyte, though commonly interlaminated with some few thin beds of sandy shale. *Scolithus* borings occur abundantly in this rock at some points; but, instead of this being the universal rule, as stated by Safford, it is the exception at outcrops thus far examined by the writer. Thickness, 2,000 feet or more. As already stated, this is considered to be the typical Potsdam sandstone.

Lowest of all the recognized Silurian, we have the Ocoee group, of the Acadian epoch, or Lower Potsdam. The beds are all more or less metamorphosed, and consist mainly of slates and conglomerates. As these are of very uniform character, and outcrop in the region of greatest disturbance and most numerous faults, it would be extremely difficult, if not impossible, to determine their actual thickness. Safford says that this *may* be 10,000 feet.

While the Chilhowee and Ocoee groups, within the Tennessee line, may especially be considered semi-metamorphic, the sandstones of the former having been well cemented without concealing or even confusing the granular structure, the shales of the latter having been squeezed into smooth slates, but showing no crystallization, and the pebbles of its conglomerates well combined with the finer paste by a sort of aqueous fusion while yet plainly showing their pebbly character; yet lesser degrees of metamorphism are plainly to be traced in many of the higher layers, far out into the Great Valley. Good examples of this are abundant in the "iron-limestone" about Knoxville, where the lines of original stratification are often much contorted, while the mass is most thoroughly compacted.

3. *From Athens to Murphy.*

As was long ago stated by Rogers, the dips, throughout large portions of the Appalachians, are mainly to the southeast, the upward displacement along the numerous lines of faults being generally on their southeastern sides. As a consequence, it is at many places difficult to recognize the successive outcrops of the different beds, until one has become somewhat familiar with their local features and forms of metamorphism. In tracing the section, however, from the railroad at Athens, Tenn., across to Murphy, N. C., and thence to Clarkesville, Ga., there appears to be less than usual of this faulting; and the equivalencies of the beds are determined with comparative ease. At Athens, the shaly, calcareous beds, with some of the heavier and more compact ones of the iron-limestone, of the Cincinnati group, form the hills, with moderate northwesterly dips. About two miles southeast, these are suddenly cut off by a fault, the Knox dolomyte abutting against them, though still showing northwesterly dips. Two miles farther on, at the crossing of Middle Creek, we pass an anticlinal, along whose back the erosion has cut slightly into the Knox shale. Knox dolomyte follows, with southeast dips, until, about two miles farther, at Jesse Dodson's, another anticlinal of the shale appears, after which regular southeast dips bring in the complete series for about four miles, the light-colored Trenton marbles appearing about nine miles out, and ten miles bringing us into a gap of a line of high red knobs formed of the iron-limestone beds. These include many thin streaks of hematite, partly in the compact "specular" form, partly oölitic, partly in a powdery or scaly condition, which has been locally mistaken for cinnabar. The hard ore can sometimes be traced, within short distances, from the "specular" condition into the unaltered oölitic "dyestone," inclosing corals and other fossils in perfect preservation, or again into a ferruginous sandstone. The ores of this belt have not yet been found in sufficiently thick beds to pay for mining, though many carloads of good ore have been gathered from the accumulations along the weathered outcrops. These iron-limestone beds recur, in several successive waves, for about four miles, one of the synclinals bending low enough to show a considerable mass of the overlying "red marble." Finally, thirteen miles out, the layers rise in sharp northwest dips, as we approach the foot of the southwestern section of the Chilhowee Mountain range. The Knox group forms the base and northwestern face of the mountain; and the heavy beds of the Chilhowee sandstone form its crest, in bold cliffs overlooking the valley to the southeast, 1,100 feet below. White Cliff Springs, of chalybeate and sulphur water, are a favorite summer resort, near the summit, and sixteen miles from Athens. Such springs are frequent

along the outcrop of this sandstone, resulting from the decomposition of the pyrite therein contained.

Looking northeastward, toward the other section of this mountain, to be hereinafter described, we see no intervening ridges and no apparent outcrop of the sandstone, but only a confused cluster of low, rounded, shale hills.

Descending the southeastern face of the mountain, we find its foot composed of the Ocoee slates, semi-metamorphosed, dipping northwest. In the middle of the valley is a knob of the Ocoee conglomerate, apparently occupying the axis of the anticlinal, since southeast dips at once recur, the slates forming the ridge bounding the valley on the southeast. The second and third ridges show small synclinal patches of the Chilhowee sandstone on their crests, with slight intervening anticlinals, while the fourth is a sharp anticlinal in the slates, which brings us to the Coca Creek waters, where the outcrop of these slates, with some small quartz veins, has yielded small amounts of placer gold. At one point here, I saw a small outcrop of the Chilhowee, dipping southeastward, and apparently cut off by a fault. Indeed, slight faults are frequent in the slates which here form the mass of the Smoky Mountains along the State line. These disturbances, however, though preventing any accurate determination of the true thickness of the beds, in no way interfere with the continuity of the mass and the constancy of the general southeasterly dips. These dips continue for about five miles beyond the State line, the beds showing more and more thorough metamorphism, the slates becoming micaceous and talcoid schists, and the inclosed beds of conglomerate becoming gneisses, more or less pebbly or even porphyritic. Here, on the eastern slope of Long Ridge, just beyond Hennegar's, northwest dips set in again, and continue about nine miles, to Davidson's, in similar beds: at this latter point, an anticlinal of softer hydromica schists and gneisses, partly staurolitic, with thin quartz veins and much iron sand, appears for half a mile, and represents the copper-bearing beds of Ducktown, the ores of which are said to be exposed in the bed of the Hiwassee, perhaps three miles west of this point. The beds appear to lie conformably beneath the true Ocoee, on both sides of the anticlinal; and there seems to be no reason for referring them to a distinct group, as was done by Emmons; and here Kerr has not followed him. The series shows a much wider outcrop at Ducktown, a dozen miles southwest, where, as is well known, rich copper mines have been developed.* The ore-deposits here are irregular masses of "stock-work," though filling crevices which run

*Still more extensive deposits would doubtless have been located, had more convenient access made mining more profitable. In the present condition of affairs, the Ducktown works, though economically managed, return hardly a living profit, and await the coming of a promised railroad for lower freights and cheaper fuel.

nearly parallel with the inclosing strata, in consequence of schistose structure especially favoring splits in that direction. At many points, they *look* like *regularly interstratified beds.* Conditions have here favored most thorough metamorphism, probably by reason of a more open and porous condition of the material, allowing more abundant percolation of the heated mineral waters. The wall-rock, at some points, is of the toughest possible quartzyte; at others, a micaceous gneiss; again, a tremolyte or hornblendyte. Both the walling and "horses" of the same material are often permeated with copper, iron and zinc sulphids. The true gangue is quartz, at some points very abundant, at others scarce.

Passing on from this belt, the southeast dips bring in again the schists and gneissoid conglomerates of the Ocoee, and the gneisses and gneissoid quartzytes of the Chilhowee and the Knox sandstones. Above this latter bed, which is here, of course, undistinguishable from the Chilhowee, the Knox shale is represented by fine-grained blue mica slates speckled with mica crystals; and, within a quarter of a mile of Murphy, we find the Knox dolomyte, in white, speckled, gray, dove-colored and nearly black marbles. These materials being so much more easily eroded and dissolved than most of the quartzose rocks, have caused the formation of a long line of valleys, and are themselves generally covered. Before treating of them in detail, or passing on to more easterly outcrops, let us review the strata along another line of approach.

4. *From Knoxville to Murphy.*

At Knoxville, just north of the railroad track, a line of fault, essentially the equivalent of, and probably continuous with, that noticed just southeast of Athens, separates the upper shales of the Cincinnati group from the Knox dolomyte, both having southeast dips. The dolomyte, with its characteristic cherts and sandstones, forms the ridge upon which the main part of the city of Knoxville stands, and extends across to the south bank of the Holston, where it is regularly overlaid by the shaly limestones of the Chazy, the fine quarry-marbles of the Trenton, and the shales and iron-limestones of the Cincinnati, which latter form a line of high red knobs, with thin streaks of compact and oölitic hematite. About three miles out, a low rounded hill, between flat shale valleys, shows a synclinal of the upper "red marble." The iron-limestone, in successive waves, then occupies the surface nearly to Rockford—say for six miles—and then yields place to the Knox dolomyte, which continues to beyond Maryville. This town, sixteen miles from Knoxville, stands near the crown of a low arch, which appears to be the equivalent of the sharp anticlinal in the valley southeast of White Cliff, though here somewhat farther out from the main

mountains. The northeastern section of the Chilhowee Mountain, indeed, is still nine miles beyond us. In approaching it, we pass over all the upper beds of the Lower Silurian, together with the shales and oölitic hematites of the Upper Silurian (Clinton, or Safford's "Dyestone group"), and the Black shale and soft gray calcareous shales of the Lower Subcarboniferous, and suddenly reach a fault of perhaps 10,000 feet, where the last-named beds abut against the Ocoee slates and conglomerates. The slates, at some points in this neighborhood, are quite chloritic, and so nearly black as to have been frequently mistaken for *anthracite*. Above the Ocoee, the mass of the mountain consists of the heavy beds of the Chilhowee sandstone, with high southeast dips. Montvale Springs, a favorite summer-resort, nine miles from Maryville, are near the foot of the mountain, and derive their chalybeate waters from the decaying pyrite of this formation. Several other strong chalybeate springs occur at various points along the mountain, but have not been similarly improved. Passing the summit, we find the southeastern foot-slopes faced with imperfect slates, representing the Knox shale, and the hollows beyond occupied by the Knox dolomyte, much disturbed and altered. The rough form of the country does not favor a full understanding of the disturbances without a thorough study of details; but it is at least evident that the dolomyte finally abuts against the Ocoee group, across a fault of several thousand feet, near the foot of the State line range, here called the Smoky Mountains. The Ocoee continues, with southeastern dips, for several miles up the Little Tennessee; but, at Rocky Point ferry, three miles beyond the line, let us cross the river, pass over a high spur to Cheowa River, and ascend the valley of that stream. The slates and conglomerates here pass rapidly into schists and gneisses. About fifteen miles from the ferry, we encounter the Chilhowee, in the form of a heavy-bedded, fine-grained gneiss, occupying, for a mile or so, the axis of a synclinal. It is reported that, on Snow Bird Creek, a western fork of Cheowa, a bed of limestone, which must be the Knox dolomyte, occupies a considerable area on the line, joining this outcrop with the synclinal noticed at Long Ridge on the other route. Northwestern dips of the Ocoee now continue on to the head of the river, consisting of hydromica schists and gneisses with staurolites and garnets, chlorite schists with garnets and quartz veins, and some bands of gneiss and quartzyte. As we approach the summit gap, toward Valleytown, we find an irregular anticlinal, equivalent to that at Davidson's on the other route, followed by very confused dips, as though the mountain bed suffered a tremendous squeezing in this region. These disturbed foldings continue even down to the valley-level, where sharp anticlinal folds occur in the micaceous gneiss, just before it dips beneath the Knox marble. It

is not improbable that careful examination might detect small patches of the marble caught and held in some of the folds high up on the mountain-side. The point at which we have now struck this formation is about seventeen miles northeast of Murphy, along the direct line of outcrop.

If, instead of crossing at Rocky Point and ascending Cheowa, we follow up the Little Tennessee, we find heavy-bedded, gray, Chilhowee quartzytes exposed as, near Hazelnut Creek, we approach and pass the synclinal just noticed as running from Long Ridge across Cheowa. Hereabouts are also laminated micaceous sandstones and chloritic slates, with veins of milky quartz. These and similar beds continue to above the mouth of the Tuckaseege, whose dip is at first pretty regularly northwest, but soon becomes irregular, turning to southwest and even to south, as if some transverse axes of fold were developed, not far to the northeastward. As we approach and pass the mouth of Nantahala River, the same beds come down again, after we pass the axis of an anticlinal about a mile below Ashe's Mill: the dips, at first nearly east, soon become S. 20° E. Along the axis of the anticlinal, which is the equivalent of that running from Ducktown past Davidson's, there are said to be copper ores in schist along Stekoa Creek. Five or six miles above the Nantahala, near Wm. Dehart's, we are supposed to reach the range of the Valley River marble, though its outcrop has not yet been reported quite so far northeast.

(To be continued.)

Knoxville, Tenn., Jan. 25th, 1875.

Art. XXXI.—*Bentham, On the recent Progress and present State of Systematic Botany.**

Had Mr. Bentham remained a little longer in the presidential chair of the Linnean Society, his recent elaborate paper would substantially have been the staple of the last of that series of annual addresses, most of which have been noticed and several reprinted in this Journal. The present report is too long to reprint, and very difficult to abridge. A sketch of the progress of the science as regarded by a botanist who personally conversed with an active correspondent of Linnæus (Gouan of Montpellier); who received useful hints on the method of botanical study from A. L. de Jussieu, the founder of the Natural System; who was in intimate relations with the elder DeCandolle, Brown, Lindley, and Hooker; who has studied in all the Euro-

* A Report made to the British Association for the Advancement of Science, 1874, separately issued, pp. 54, 8vo.

pean herbaria, and been in correspondence with all and in personal acquaintance with most of the notable botanists of the last forty or fifty years, and, finally, who is at the present time the most productive and the soundest of systematic botanists, cannot be otherwise than replete with interest.

We will pass over Mr. Bentham's retrospect of the first great movement of modern botany over the Linnæan thoroughfare to a knowledge of genera and species, and also of that of his and our own days, by which we have ascended a higher platform of ordinal classification along the path, once so difficult, but now made available and easy through the labors and genius of Jussieu, Brown, DeCandolle, and other but less illustrious pioneers; and we come down to "the next period in the progress of systematic botany, the seventeen years that elapsed from 1832 to 1859."

"The change from the technical to the scientific study of plants, which, during the preceding period, had been working its way through so many obstacles, was now complete. The Linnæan platform, established on the relations of genera and species, had now been so long and so universally adopted as the basis or stand-point, that the credit due to its founder was almost forgotten in the triumphant destruction of the sexual scaffolding he had erected for the ascent of the higher stages, and now completely superseded by the progress of the Jussiæan roads, although it was chiefly by the consistent following out of the principles laid down by Linnæus himself that the change had been effected. No would-be botanist was allowed any longer to eschew the labor of the methodical study of plants, or to indulge in the belief that their technical sorting constituted the science. * * * * *

"It would seem that at this advanced stage of our progress the guide-posts of the principal paths had become so firmly established, the principles upon which plants should be scientifically classed so clearly laid down, and so far carried into practice, that little remained to be done toward completing the survey of the territory—toward a general distribution of species according to their natural affinities—beyond the more accurate delineation of details and the interpolation of newly-discovered species; and that the systematic botanist could already look toward that summit upon reaching which his labors in aid of the general advance of the science might come to a close.

"But there was a rock ahead which had long been looming in the distance, and which on a nearer approach opposed a formidable obstacle. What is a species? and what is the meaning of those natural affinities according to which species are to be classed? were questions which in 1859 it was generally thought vain to discuss, or the answers to which, given to us by doctrinal teachers, unsupported by or independent of facts,

it was considered as sacrilegious to doubt. We were taught, and some may still believe, that every species, such as we now see it, was an original creation, perpetuated through every generation within fixed limits which never have been and never will be transgressed. We were less authoritatively told that resemblances of different species were owing to their having been formed upon one plan variously modified. To the question why they were so modified, the ready answer was, such was the will of the Creator; and in order not to suppose that that will was influenced by mere caprice, it was suggested that the modifications were either to suit the plant to the circumstances it was placed in, or to remedy defects in the original plan, or we were simply told that the subject was beyond our powers of comprehension.*

"One consequence of this apparent impossibility of proceeding further in the investigation of the causes of affinities and of this necessity of taking species as separate creations in enormous numbers, with resemblances and differences in endless variety according to the inscrutable will of the Creator, was the encouragement it gave to arbitrary classifications and interminable disputes as to the limits of individual species. It was, indeed, generally admitted that plants should be arranged in genera, orders, &c., in groups of higher and higher grades according to the importance of the characters they had in common, and that the test of species was the persistence of its characters through two or more generations; but there were no means of estimating the importance or value of characters except by such vague standards as the number of species in which they had been observed to prevail, no means of determining what degree of variation and persistence actually distinguished the species from the variety. The botanist who affirmed that *Rubus fruticosus*, *Draba verna*, or *Sphagnum palustre* were each one very variable species, and he who maintained that they were collective names for nearly four hundred, for at

* "In my frequent intercourse during the above period with foreign botanists, I heard more than one German Professor affirm that a type-form was created for each natural order (the common clover, for instance, being that for Papilionaceæ), that Nature set to work to modify this type-form in framing species of a more complicated structure, till, tired of the exertion, she next produced new species by the simple omission of some of the complications. A French botanist of great eminence, to account for the number of plants in cultivation which are not known to exist in a wild state, observed that we could not suppose that man would have been created without a simultaneous creation of plants for him to cultivate for food, quite independent of the wild vegetation which existed before him for the food of animals. And many other still wilder theories were propounded to account for facts inconsistent with the presumed independent creation and absolute fixity of species. The best authorities went no further than defining affinity as correspondence of characters, physiological or structural, and estimating the value of characters and the importance of peculiarities or modifications of character according to their known connexion with the phenomena of life."

least two hundred, or for some twenty separately created and invariably propagated species, had each arguments in their favour to which no definite reply could be given; and systematic botany was in too many cases beginning to merit the reproach of German physiologists, that it was degenerating into an arbitrary multiplication and cataloguing of names and specimens, of use to collectors only, and serving as impediments instead of aids to the extension of our scientific knowledge of the vegetation of the globe.

"It is true that long before the period under consideration some indications by which this great obstacle to further progress might be surmounted had been vaguely given, and the theory of a common descent of modern species had been broached, or generally proposed as a solution of some of the difficulties; but not in a manner sufficiently plausible to overcome the prejudices against following up any such track, nor supported by facts and observations sufficient to awake the attention of the most anxious pursuers of the science. It was reserved for the publication of the 'Origin of Species' in 1859 to mark out a practicable path by which the higher summits might be attained. The doctrine of evolution of species, according to laws originally fixed, instead of arbitrary intervention upon each and every occasion, was in this remarkable work clearly traced out, supported by powerful arguments, and founded upon facts and observations the accuracy of which no one could doubt; and a way was thus opened up to a pinnacle, which in a wonderful degree enlarged the range of vision of those who had the courage to follow its propounder up the giddy height. It was immediately and successfully taken to by several of the most eminent of our naturalists accustomed to philosophical deductions from ascertained facts; it was blindly accepted, but misused, by some German and Italian speculators, who, in their hurry to adopt Darwinism before they well understood it, and in their eagerness to go beyond the point to which the road had been securely marked out by the author, or to diverge into by-paths which led to precipices and pitfalls, added to the alarm of the timid; whilst it was not only shunned, but denounced as fraught with the utmost danger by the great majority who were accustomed to place tradition above reasoning. We systematists hesitated at first to advance in a direction so contrary to that which we had determinately followed for so long a period; but after a careful study of the facts and arguments upon which the new course was founded, and of the guide-posts which had been set in it, we most of us have felt but little doubt of its safely leading us over difficulties, which we had so long reckoned as insurmountable, into a vast and entirely new field of observation, calculated to give a stability to the results of our labours, of which

we had hitherto formed no conception. The last of the eminent observers of nature who persistently maintained the independent creation and absolute fixity of species (the late distinguished Professor Agassiz) has recently gone from among us; and it may now be given as a generally received doctrine, that all natural methods must be founded on affinities as dependent on consanguinity. Fifteen years have sufficed to establish a theory, of which the principal points, in as far as they affect systematic botany, may be shortly stated as follows:—

"That although the whole of the numerous offspring of an individual plant resemble their parent in all main points, there are slight *individual* differences between them.

"That among the few who survive for further propagation, the great majority, under ordinary circumstances, are those which most resemble their parent, and thus the *species* is continued without material variation.

"That there are, however, occasions when certain individuals with slightly diverging characters may survive and reproduce races in which these divergences are continued even with increased intensity, thus producing *Varieties.*

"That in the course of an indefinite number of generations circumstances may induce such an increase in this divergency, that some of these new races will no longer readily propagate with each other, and the varieties become *New Species*, more and more marked as the unaltered or less altered races, descendants of the common parent, have become extinct.

"That these species have in their turn become the parents of groups of species, i.e., *Genera*, *Orders*, &c., of a higher and higher grade according to the remoteness of the common parent, and more or less marked according to the extinction or preservation of unaltered primary or less altered intermediate forms.

"As there is thus no difference but in degree between a variety and a species, between a species and a genus, between a genus and order, all disputes as to the precise grade to which a group really belongs are vain. It is left in a great measure to the judgment of the systematist, with reference as much to the use to be made of his method as to the actual state of things, how far he should go in dividing and subdividing, and to which of the grades of division and subdivision he shall give the names of Orders, Suborders, Tribes, Genera, Subgenera, Sections, Species, Subspecies, Varieties, &c., with the consequent nomenclature. In the limitation of his orders, genera, species, &c., he must carefully observe those cases where the extinction of races has definitely isolated groups having a common parentage; and in other cases where the preservation of intermediate forms has left no such gaps, he is compelled to

draw arbitrary lines of distinction wherever it appears to be most convenient for use. In the pre-Darwinian state of the science we were taught, and I had myself strongly urged, that species alone had a definite existence, and that genera, orders, &c., were more arbitrary, established for practical use, and founded on the combination of such characters as appeared the most constant in the greater number of species, and therefore the most important. We must now test our species, as well as genera or other groups, by such evidences as we can collect of affinity derived from consanguinity.

"In valuing these evidences, in estimating the comparative value of characters, a new difficulty has arisen, that of distinguishing the two classes of characters to which Professor Flower has appropriately given the names of *essential* and *adaptive*,—the former the result of remote hereditary descent, the latter the more recent effect of external influences. This distinction is often the more difficult, as the essential ones are often only to be found in embryos, in the early stages of organs, or are merely indicated by slight rudiments requiring close observation to detect them; whilst the adaptive ones, of comparatively small systematic importance, are often developed in external form, in ramification, spinescence, foliage, &c., and are the most striking to the eye. One consequence is, that the systematist of the present day sees more and more the necessity of preparing a double arrangement of his genera, species, and other groups—a natural one according to the best evidences of affinity for the purpose of scientific study, and an artificial clavis by which the student can be led to identify genera or species by the more readily observed characters, which may only form part, or be but chance accompaniments, of the essential ones. The greatest change, however, which the adoption of the doctrine has effected in the methodical study of plants is the having rendered it necessary, in the case of every genus or other group, to take into account and specially to estimate the value of all the characters observed—no one can be taken as so absolute as to obviate the need of considering others, no one can be passed over as theoretically worthless; and whilst this adds immensely to the labour of the systematist and to the calls on his judgment, it gives equal increase to the value of the results obtained."

The above statement, as we believe, clearly expresses the general view of the leading systematic botanists of our time, as to the derivation of species, as well as some of the grounds which have inevitably led to it. As by Mr. Bentham in England, so also here, such views were early reached, not through general theorizing nor under partizan predilection, but upon sober consideration of the facts that we knew

most about, and to which years of impartial study have been given. Long-continued conscientious work in systematic botany, upon copious materials, gives excellent training; and it is pretty sure to lead most minds to the conclusion that the present forms came by way of derivation and diversification. The results of the study of the Tertiary and Cretaceous fossil botany of the northern hemisphere bring confirmation which every year makes stronger; and the riddles of geographical distribution finding easy solution thereby, the adoption of the hypothesis as the practical one seems unavoidable, if there is to be any science in botany.

[To be continued.]

ART. XXXII.—*Spectroscopic Examination of Gases from Meteoric Iron;* by ARTHUR W. WRIGHT.

THE well-known investigation of Professor Graham, upon the gases occluded by meteoric iron, showed that the Lenarto meteorite contained at least 2·85 times its volume of gaseous substances, consisting of hydrogen, carbonic oxide, and nitrogen.* The percentages of the several gases were: hydrogen, 85·68; carbonic oxide, 4·46; nitrogen, 9·86. As he had observed that it is difficult to produce an absorption by iron of more than its own volume of hydrogen under a pressure of one atmosphere, he concluded that the Lenarto iron had derived this gas from an atmosphere containing it in abundance, and at a pressure much greater than is found at the surface of the earth, such, for instance, as must exist near the surface of the sun or of the larger fixed stars.

Similar results were obtained subsequently by Professor J. W. Mallet,† in experiments upon a meteoric iron from Augusta Co., Virginia, in which he found the same gases present, in a somewhat different proportion, with the addition of a small amount of carbonic di-oxide, the percentages being: hydrogen, 35·83; carbonic oxide, 38·33; carbonic di-oxide, 9·75; nitrogen, 16·09. The percentage of hydrogen in the first portion of gas collected was greater than in the portions obtained later. The volume of the gases was 3·17 times that of the iron.

On the supposition that the meteoric iron has received its hydrogen and other gases from an extra-terrestrial source, if not from the sun, possibly from some other body having a similar atmosphere of great density, it seemed probable that the unknown gaseous elements, assumed to be present in the solar corona and chromosphere, might be detected in the gases

* Proc. Royal Soc., xv, 502. † Ibid., xx, 365.

yielded by meteorites; and this investigation was undertaken in the hope that the spectroscope would reveal them if present, though their small amount or peculiar character should render their discovery by the ordinary chemical means difficult or impossible.

The mode of obtaining the gas was similar to that pursued by Graham and Mallet, with the exception, that the iron, instead of being a single solid mass, was in the form of fine chips, prepared by boring into the meteorite with a steel drill upon a lathe, those portions which came from near the exterior being rejected, with the observance of suitable precautions to prevent the admixture of foreign matter. The fine particles of the iron were placed in a thick tube of very hard glass, which had been carefully cleaned, and were pressed into it with a glass rod. It was found possible to bring the glass tube, thus partially filled, to a red heat, without its yielding very much to the atmospheric pressure.

For the exhaustion a very efficient Sprengel pump was used, provided with a U-gauge, the limbs of which have an internal diameter of about six millimeters, and which is capable of giving accurate readings to a fraction of a millimeter. The mercury is supplied to the fall-tube by a recurved tube, the branches of which are thirty-two inches long, an arrangement which effectually prevents the entrance of air. The pump is capable of carrying the exhaustion to such a point that it becomes difficult to estimate the difference of level of the mercury surfaces in the gauge, and the electric discharge through a vacuum-tube exhausted by it becomes very feeble. That there was no leakage was shown by the fact that, after the exhaustion had been carried to the extreme, the gauge maintained its position unchanged for days.

To the exhaust-tube of the pump was attached a horizontal glass tube having two branches at right angles to its length. To one of these a vacuum-tube of the form ordinarily employed for spectroscopic work was attached, so as to have a vertical position. The other was closed by a stop-cock by which air or any gas could be admitted. The outer end of the horizontal tube was also provided with a stop-cock, and beyond this the tube containing the iron was firmly attached with cement.

It was important to know what lines would appear in the spectrum from an ordinary tube exhausted by the pump, and containing air, in order to judge correctly as to the presence or absence of any gaseous bodies coming from the iron. It is well known that the lines of hydrogen, and bands due to carbon compounds,* appear when the air in the tube is rarefied to a high degree. A number of preliminary trials showed that

* Plücker and Hittorf. On the Spectra of Ignited Gases and Vapors, Phil. Trans. Royal Soc., vol. 155, p. 1.

these lines, with very faint carbon bands, probably due to fatty matters employed in lubricating the stop-cocks, invariably appeared toward the limit of the exhaustion, while lines due to the mercury were generally visible during the later stages of the rarefaction, having greater or less brilliancy according to the temperature of the room. The red hydrogen line is frequently dimly visible when the tension has fallen to four or five millimeters, but is neither distinct nor relatively bright until the pressure is much less than that, while the other hydrogen lines appear distinct only after a much higher degree of rarefaction is attained.

The first trial was made with a piece of meteoric iron, which is a fragment of the great Texas meteorite, in the cabinet of Yale College. This meteorite, of which a description has been published,* is a large mass weighing 742 kilograms, having the following composition: Fe, 90·91; Ni, 8·46; insoluble portion containing some carbon, 0·50.† The chips produced by the borer were mostly very small particles. Much of the metal was reduced to powder, and the coarser portions were crushed in the process of boring, so as to destroy the solidity of the iron and break up its structure. A quantity of the borings representing 0·384 cubic centimeter of the solid iron were placed in the glass tube, which was then fastened in its place. The stop-cock was closed and the pump set in operation. When the gauge stood at 17 mm. the cock was momentarily opened, and was then closed. The gauge rose to about 100 mm., and when the pump had brought it down again to 17 mm. the spectroscope was applied to the vacuum-tube. The red hydrogen line was seen bright, the rest of the spectrum having the ordinary banded structure due to nitrogen and oxygen. As the exhaustion proceeded the other hydrogen lines appeared, and when the tension was reduced to 4 mm. both H_α and H_β were bright and distinct. H_γ was visible, though less prominent. The carbon bands also were distinctly seen. At 2·5 mm. pressure the stop-cock was opened, causing the gauge to rise 12·5 mm., after which it remained nearly stationary for fifteen minutes, although the pump was in action. A simple calculation shows that the first rise of 12·5 mm. is just what should have been produced by the air contained in the tube with the iron. But the fact that the gauge maintained this position for a considerable time, while the pump was continually withdrawing the air, shows that the iron gave off a portion of its gas without the application of heat, and it was repeatedly observed, in other experiments, that when the stop-cock was open, and the pump not in action, the gauge continued to rise very slowly, sometimes as much as two millimeters in an hour or two.

* Profs. B. Silliman and T. S. Hunt, this Journal, II, ii, 370. † Loc. cit.

A gentle heat was now applied to the tube containing the iron, by means of a Bunsen burner. This brought the gauge in a few minutes to about 6 mm., and produced a marked change in the appearance of the vacuum-tube, which before had the appearance of an ordinary hydrogen tube. The light in the broad portion became a straight, hazy stream of a dull greenish-white color, very similar to that given by a tube containing either of the oxides of carbon. After the tube had been exhausted to 2 mm., heat was again applied rather more strongly than before, but still below redness, carrying the gauge to 9 mm. in about ten minutes, the effect upon the spectrum being merely to increase the intensity of the carbon bands. The tube was now wrapped with copper foil, and the temperature, by means of a Bunsen flame, carried to low redness, so that the glass softened and began to yield. But a small quantity of gas was given off, the gauge at the end of ten minutes standing at 5 mm. The stop-cock being closed, the exhaustion was continued to 1·5 mm. At this point the spectrum was nearly the same as before, but was somewhat less brilliant. Certain other lines appeared in the spectrum, of which mention will be made in a later paragraph.

A second set of experiments was made with a specimen of meteoric iron from Tazewell County, Tennessee. The meteorite, of which this was a portion, weighed when found twenty-five kilograms, and has the composition, Fe, 83·02; Ni, 14·62; other substances, 1·93. No carbon was found in it.* The iron was found to be softer than the previous specimen, and was bored with comparative ease, the fragments produced being coarser than those from the Texas iron. The experiments were made in the same manner as in the previous examination, 0·634 cubic centimeter of the iron being placed in the glass tube upon the pump, and the stop-cock closed. After exhausting to one millimeter, the cock was opened for a moment to withdraw the air from the specimen-tube, and then closed. This produced a rise in the gauge of 104 mm. On again exhausting to one millimeter, and opening the stop-cock, the gauge rose 20 mm., not coming to rest immediately, but continuing to ascend slowly for some time. From calculation it appears that air at a tension of 105 mm. in the tube containing the iron would raise the mercury in the gauge about 15 mm. only, the remaining effect evidently arising from evolution of gas from the iron. This the spectroscope confirmed, as the red and green hydrogen lines were brilliant, even before the pump had further reduced the pressure, and they increased in relative intensity as the rarefaction increased. The carbon bands, if present, were not especially noticeable, as there is no record of them in the notes of the experiment.

* Prof. J. L. Smith, this Journal, II, xix, 153.

The tube containing the iron being strongly heated, gas was given off, which brought the gauge to 29 mm. in about 15 minutes. The heat was such as to redden and soften the glass. The vacuum tube, when the discharge was passed through it, appeared like an ordinary tube containing carbon compounds, and the spectrum gave the hydrogen lines very brilliantly, with the four chief carbon bands in great strength. The cock was again closed and the pump set in operation, the spectrum being observed from time to time throughout the process of exhaustion. As the tension of the gas decreased, the hydrogen lines became relatively stronger, and the carbon bands grew narrower; the one in the red beginning to be resolved into lines when the pressure was much reduced. At 1 mm., the carbon bands were still prominent, and some narrow bands belonging to nitrogen were observed.

In the first of the specimens of iron thus far examined. the proportion of carbon found by chemical analysis was very small, and in the second none at all was detected. The third series of experiments was made with an iron containing a larger amount of carbon. This was a fragment of the well-known meteorite from Arva, in Hungary, which has the following composition: Fe, 90·471; Ni, 7·321; residuum containing carbon, silica, and cobalt, 1·404.* On attempting to bore into this iron, as had been done with the others, great difficulty was experienced, as the metal has nearly the hardness of steel. After the expenditure of much labor, a sufficient quantity of the iron was obtained for the examination, representing 0·224 cubic centimeter of the solid metal. It was in a state of minute subdivision, being almost entirely reduced to fine powder. After it was placed in the tube connected with the pump, the air was exhausted from the latter as long as any change was visible in the gauge, bringing it finally very nearly to zero. The stop-cock had been closed from the first. The work having been interrupted for several days, the gauge was found, on resuming it, not to have suffered the slightest change, showing that the joints of the pump were perfectly tight. The gauge rose, on opening the stop-cock, to 98 mm. As the gases were rarefied by the pump, the hydrogen lines shone out very brilliantly when the pressure was reduced to a few millimeters, and at 2 mm., the vacuum tube had to the eye the appearance of an ordinary hydrogen tube while the electric discharge was passing through it. At this pressure, the second and third carbon bands, counting from the red end, were quite bright. The pump was stopped when the tension was reduced to 1·5 mm., but the gauge rose very slowly, gaining about a millimeter in three hours. The spectrum was as before except that

* Analysis of A. Löwe. Notice in this Journal, II, viii, 439.

the first carbon band was now visible, and the others were stronger. There could be no question as to the evolution of both hydrogen and the carbon gases, without the application of heat, by simple diminution of the atmospheric pressure.

A very gentle heat was applied to the tube for a few minutes, making it hardly warm enough to give pain when touched by the hand. This had the effect to raise the gauge to 28 mm. in fifteen minutes, and cause an entire change in the appearance of the vacuum-tube, which became greenish in the broad part, like those in the former experiments. The spectrum, when the pressure was reduced to a few millimeters, showed the carbon bands quite bright, and as the pressure decreased the first one became clearly resolved into five fine lines nearly equidistant, upon a faintly luminous background.

A somewhat stronger heat was now brought to bear upon the tube, but far less than would have been necessary to bring it to redness, as it was desired to avoid the possibility of error from the oxidation of the carbon by traces of oxygen left in the tube. The gauge rose to 50 mm., sinking to 47 mm. as the tube regained its ordinary temperature, doubtless from the contraction of the gas on cooling; for, on subsequently exhausting, it was found that the gauge continued to rise, though with extreme slowness. The spectrum did not differ essentially from that before observed, except in the greater intensity of the carbon bands. A few cubic centimeters of the gas, collected during the second heating, when tested qualitatively, gave evidence of the presence of hydrogen, carbonic oxide, and carbonic di-oxide. The proportion of the latter did not appear to be more than three or four per cent, that of the carbonic oxide being probably greater. The gas burned with a flame like that of hydrogen. The tube was subsequently heated to redness, and kept at that temperature until the gauge became very nearly stationary, the change in it being so slow as to be scarcely perceptible. Enough gas was given off to raise the mercury to 33 mm., after which the heat was discontinued.

For the sake of comparison, a trial was made with borings from a ball of soft iron. The ball had a diameter of three centimeters, and was solid. The exterior was covered with rust. The examination was made in the same manner as in the previous cases, excepting that the stop cock remained open during the process of exhaustion. When the tension was reduced to 9 mm. the red and green hydrogen lines were brilliant, and the carbon bands appeared, with feeble intensity, as the rarefaction increased. On heating the tube strongly, though not to redness, gas was given off in abundance, raising the mercury in the gauge to 28 mm. in a few minutes, and causing the vacuum tube to assume the appearance of an ordinary CO or CO_2 tube.

The spectrum also showed a great increase in the intensity of the carbon bands. It was in every way similar to that observed in the case of the meteoric irons, except that the relative intensities of the parts due to hydrogen and the oxides of carbon were different, indicating a much smaller proportion of the former. The hydrogen lines neither appeared so early, nor were they so bright as in the former experiments.

Although foreign to the main purpose of the investigation, it seemed of interest to determine the amount of the gaseous products yielded by the three specimens of meteoric iron. By measurement it was found that, when the tubes containing the iron borings were attached to the pump, the gauge was lowered 10·4 millimeters for every cubic centimeter of gas carried out by the mercury. It was thus easy to find the amount of gas given off by simply observing the reading of the gauge. From calculations made in this way it was ascertained that the Texas iron gave off 4·75 times its volume of the mixed gases, and the Tennessee iron 4 69 volumes. Although it was evident that the greater portion of the gas had been driven off by a comparatively moderate elevation of temperature, the whole amount was by no means exhausted, as the heat was withdrawn before the evolution of gas had entirely ceased. These volumes, it will be observed, are considerably larger than those obtained by Graham and Mallet from the specimens examined by them. This is probably due to the minute subdivision of the iron, which would favor a more rapid and complete evolution of the gas. It was found, in their experiments, where the iron was in a single solid mass, that gas was still given off after the high temperature had been continued for several hours.

In the case of the iron from the Arva meteorite, which was in the state of fine powder, the yield was very much greater, a little more than thirty volumes of gas being given off in the experiments already described, in which the highest temperature was far below a red heat. On subsequently heating the tube and its contents to low redness fourteen volumes more came off, making the whole amount somewhat more than forty-four volumes, not reckoning the additional quantity gained in a subsequent experiment. It seemed not impossible that a portion of this large amount of gas might be atmospheric air condensed upon the fine particles of iron, but it was difficult to determine this point with certainty. After the last heating just mentioned, the iron was removed from the tube, exposed to the air for several hours, and then replaced. On re-heating it very strongly a considerable amount of gas was evolved, but the spectroscope, when applied to the vacuum-tube, showed the presence of hydrogen and the carbon compounds as before, making it probable that the increase due to condensed air was

not large. The recent experiments of M. Cailletet* show that ordinary iron, under certain conditions, as when electrolytically deposited, may absorb nearly two hundred and fifty times its volume of hydrogen.

The descriptions of the spectra thus far given have reference to those lines and bands which were caused by the presence of hydrogen and the oxides of carbon. The vacuum-tubes exhibited differences depending chiefly upon the relative proportions of these gases present, and which have already been sufficiently noted. In the spectra from all the tubes there appeared a number of other lines, which were found to belong, for the most part, to oxygen. The tube containing gases from the Tennessee meteorite gave also several lines due to nitrogen. The latter were somewhat different, as to the order of their relative intensities, from those observed with nitrogen alone. None of them were very bright, and it was a matter of some difficulty to fix their positions with entire accuracy by measurement. The observations were made with a spectroscope of six prisms, with a repeating prism, giving the dispersion of twelve in all. One of the lines appeared to coincide with the chief coronal line 1474 K., but it is not so sharp as the latter appears in the solar spectrum. A narrow and rather faint band appears at this point also in the spectrum of the electric spark between platinum points in air. Of the oxygen lines, one falls some distance below 1474, having the position 1462 K., very nearly. This, though not brilliant, is the strongest line in that region of the oxygen spectrum as obtained from a vacuum-tube. It falls very near the position assigned to a bright coronal line by the Italian observers, Denza and Lorenzoni, of the eclipse of Dec. 22, 1870.† The former gives 1463 K. as the approximate position, while the latter assigns the same number, with the remark that the probable error of the reading is four divisions of Kirchhoff's scale. A second line of oxygen, less bright, but sharp and distinct, has the position 1359 ± 1 K. A coronal line, which also appears in the auroral spectrum, has, from Prof. Young's observation, the position 1350 ± 20 K., but the limits thus stated are too wide to give the supposition of a coincidence between it and the oxygen line anything more than a doubtful probability.

These lines of oxygen and nitrogen in the spectra observed are somewhat variable in their character, doubtless owing to the presence of the hydrogen, for the same appearances can be reproduced substantially by employing a mixture of these gases. Several experiments were made with a tube containing at first only air. When this was exhausted the bands arising from

* L' Institut, Nouv. Sér., Ann. 3, p. 44.

† Rapporti sulle Osservazioni dell' Ecclisse totale di Sole, dell 22 Dicembre, 1870. pp. 62, 106.

the compounds of nitrogen and oxygen appeared as usual, but on admitting a little hydrogen the spectrum was completely changed, and lines appeared, belonging to oxygen and nitrogen, the one or the other, or both, according to the amount of the gases present, and varying with the temperature and pressure. Lockyer and Frankland, in their experiments upon mixtures of hydrogen and nitrogen,* found that the result was not a mere superposition of the spectra of the two gases, but that the presence or absence of lines, and their relative intensities, depended upon the proportions of the two gases, as also upon the degree of rarefaction, and the energy of the electric discharge.

It will be seen, then, that the investigation, so far as the presence of new substances in the gases of meteoric iron is concerned, gives only a negative result. But the fact, incidentally observed, of the varying character of the oxygen and nitrogen lines in the presence of hydrogen, and the near coincidence of two of them with prominent coronal lines, with the possible coincidence of a third line, goes to show that the characteristic lines in the spectrum of the corona, so far from indicating the presence of otherwise unknown elements, are due simply to hydrogen and the gases of the air, namely, oxygen and nitrogen.

Yale College, March 18, 1875.

Art. XXXIII.—*The Discovery of the duplicity of the principal Star of* Σ 1097; by S. W. Burnham.

The naked-eye star, B A. C. 2470, is given in *Mensuræ Micrometricæ* as a wide pair, the secondary being of Struve's 8·7 magnitude. The following measures have been made:—

Struve	(1832·1)	D = 20″·20	P = 312°·1
Smyth	(1834·1)	20·00	315·0
Radcliffe	(1863·1)	20·52	311·2

Sir John Herschel subsequently noted another companion of the 12th magnitude at an estimated distance of 25″. A short time since I examined this with my 6-inch Clark refractor to get the position angle of the third star, which Herschel has not given, and perceived at once that the bright star of the group was itself a close double. The components are separated only about 0″·7, and considerably unequal, being 6 and 7½ magnitudes. No suitable opportunity has since occurred to measure the angle, but the close pair and the distant companion appeared to be almost exactly in the same line. Taking the latest measures of Struve's pair, with the estimated distances and angles

* Solar Physics, p. 530.

of the new and distant companions, the system would stand as follows:—

A and B	D = 0".7 ±	P = 160° ±
AB and C	20·52	311·2
AB and D	30· ±	160· ±

The star, D, is very nearly one and a half times the distance of C. With a power of 400, my instrument just separates the close pair.

Chicago, March 13th.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Ethyl Phosphite and the Constitution of Phosphorous Acid.*—Under the direction of Professor Wislicenus, Zimmermann has made some experiments upon ethyl phosphite, with a view to unravel the constitution of phosphorous acid. The ether was prepared by a modification of Railton's method, i. e., by acting upon sodium ethylate with phosphorous chloride, special precautions being taken to ensure the dryness and purity of the materials. The yield was from 65 to 70 per cent. The properties of the ethyl phosphite agreed with those described by Railton; it was a disagreeably smelling liquid of 1·075 sp. gr., boiling in the air at 191°, soluble in water, alcohol, and ether, and slowly decomposing in contact with the air. On analysis it yielded numbers corresponding to the formula $PO_3C_6H_{15}$. Upon saponification and precipitation with barium chloride, a barium phosphite corresponding to the formula $(BaHPO_3)_2+H_2O$ was obtained, thus proving that the ether had not the composition $C_2H_5PO(OC_2H_5)_2$. No ethyl phosphite was formed. Oxidation with nitric acid gave phosphoric and oxalic, but no ethyl-phosphoric acid. Oxidation in the air converted it into ethyl phosphate. Heated in a sealed tube to 250° for ten hours, hydrogen phosphide and ethylene gases were evolved, and phosphoric acid, but no triethyl phosphine remained in the liquid. The reaction appears to be

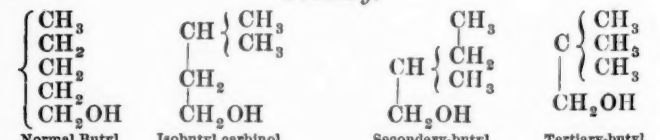
$$(P(OC_2H_3)_5)_4 = (PO(OH)_3)_3+PH_3+(C_2H_4)_3.$$

On treating the pure crystallized acid with sodium hydrate in excess, and adding absolute alcohol, a syrup was precipitated which, after washing, showed a weak alkaline reaction. Placed in a vacuum over sulphuric acid, it showed no tendency to crystallize even after months. Upon analysis it yielded phosphorus and sodium in the ratio of 1:3. The author believes consequently in the existence of tribasic phosphorous acid, the normal acid of trivalent phosphorus, and a trihydroxyl derivative.—*Liebig's Ann.*, clxxv, 1, Dec. 30, 1874. G. F. B.

2. *New Salts and Reactions of Cæsium and Rubidium.*—Some time ago GODEFFROY described the precipitate produced in cæsium solutions by antimonous chloride. (To this he now assigns the formula $(CsCl)_6SbCl_3$.) He has continued his investigation and now announces that not only antimonous chloride, but the chlorides of many other metals give, with cæsium salts, crystalline precipitates, difficultly soluble in hydrochloric acid. He mentions of these: iron-cæsium chloride $Fe_2Cl_6(CsCl)_6$; bismuth-cæsium chloride, $BiCl_3(CsCl)_6$; zinc-cæsium chloride, $ZnCl_2(CsCl)_2$; cadmium-cæsium chloride, $CdCl_2(CsCl)_2$; mercuric-cæsium chloride, $HgCl_2(CsCl)_2$; copper-cæsium chloride $CuCl_2(CsCl)_2$; manganese-cæsium chloride $MnCl_2(CsCl)_2$; and nickel-cæsium chloride, $NiCl_2(CsCl)_2$. These salts fall readily as precipitates when the metallic chloride, dissolved in concentrated hydrochloric acid, is mixed with a similar solution of cæsium chloride. The double salts are very soluble in water and in dilute hydrochloric acid, but crystallize out easily on evaporation. An investigation was then made into the behavior of the other alkali-metals and of ammonium with the metallic chlorides above mentioned, but no results were obtained with any of them except rubidium, which gave with the chlorides of antimony, bismuth, iron, zinc, cadmium, mercury, copper, manganese and nickel, beautifully crystallized double salts, completely analogous with the cæsium double chlorides in composition. Since these cæsium salts are not perfectly insoluble except in concentrated acid, only the platino-chloride can be used for its separation.—*Ber. Berl. Chem. Ges.*, viii, 9, Jan., 1875. G. F. B.

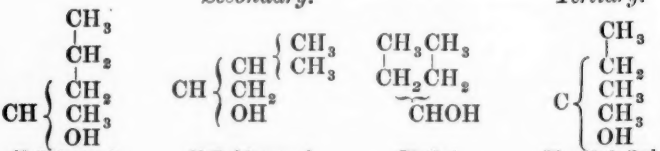
3. *On Diethylcarbinol, a new Isomer of Amyl alcohol.*—The present theory of chemical constitution points out the existence of eight isomeric amyl alcohols, of which four are primary, three are secondary and one is tertiary. These are constituted as follows:—

Primary.



Normal Butyl carbinol. Isobutyl carbinol. Secondary-butyl carbinol. Tertiary-butyl carbinol.

Secondary. *Tertiary.*



Methyl-propyl carbinol. Methyl-isopropyl carbinol. Di-ethyl carbinol. Di-methyl-ethyl carbinol.

Of these alcohols, the first discovered and the best known is the one produced during the fermentation of grain, potatoes, beet

roots, etc., and known as fusel oil. It was discovered by Dumas in 1835 and its constitution was fixed by Erlenmeyer in 1867, by proving that it yielded on oxidation a valeric acid identical with one prepared by him from isobutyl cyanide, and also with the isopropylacetic acid of Frankland. It is therefore the second of the alcohols above given, viz., isobutyl-carbinol. Wurtz, in 1862, prepared a second amyl alcohol from amylene, which was proved subsequently, both by himself and Kolbe, to be the sixth alcohol above given, viz., methyl-isopropyl-carbinol. The third amyl alcohol was produced synthetically by Popoff in 1867, by acting with zinc ethyl on propionyl chloride. This synthesis, as well as its oxidation products, proved it to be the eighth, or tertiary, alcohol given above, viz., di methyl-ethyl-carbinol. Wurtz in 1868 produced a fourth isomer, which he called ethyl-allyl-hydrate, since it was prepared by the action of zinc ethyl on allyl iodide. It corresponds to formula number five in the above list, being methyl-propyl-carbinol. The fifth isomer was prepared in 1870 from normal butyl cyanide, by Lieben and Rossi. Its mode of preparation as well as its oxidation product, normal valeric acid, prove it to be the first of the series, or normal butyl-carbinol. Only three of the possible amyl alcohols remain, therefore. WAGNER and SAYTZEFF now communicate the discovery of one of the two remaining secondary amyl alcohols, viz., di-ethyl-carbinol. The method by which this was accomplished, which is general, was suggested by the fact discovered by Frankland and Duppa, that, while the action of an organo-zinc compound upon oxalic ethers caused the substitution of a hydrocarbon radical for oxygen in one of the carboxyl groups, a further similar substitution in the second carboxyl group could not take place; a condition being produced by the first substitution, which is unfavorable to the second. Hence the facility of substitution of the oxygen of a carboxyl group by a hydrocarbon radical belongs only to those ethers in which the carboxyl group is not in combination with any radicals, the carbon and hydrogen being directly united. For these reasons, zinc ethyl should act upon ethyl formate to produce a compound which, acted on by water, should yield a secondary amyl alcohol. Experiment fully confirmed this view. A series of trials led to the adoption of the following method of procedure: In a flask connected with an inverted condenser are placed four molecules of ethyl iodide, one of ethyl formate, a small quantity of zinc-sodium, and enough granulated zinc to fill the flask to the level of the liquid. The whole was heated on the water bath, under slight pressure, until it ceased to boil and no more gas was evolved. After cooling the crystalline mass was treated with iced water, then with hydrochloric acid to dissolve the zinc hydrate, and submitted to distillation. From the distillate the diethyl-carbinol was obtained, as an imperfectly mobile liquid, having the irritating odor belonging to common amyl alcohol. Its sp. gr. is 0·822 at 0°, and it boils at 116°–117°. On oxidation it yields diethylketone. Several of its derivatives are described. The authors have also

produced secondary butyl alcohol by the new method.—*Liebig's Ann.*, clxxv, 351, Jan., 1875. G. F. B.

4. *Production of Allyl alcohol in the dry Distillation of Wood.*—ARONHIEM has discovered allyl alcohol among the products of the dry distillation of wood. The crude wood spirit—which owes its penetrating odor to this substance—after distillation of the methyl alcohol, leaves a residue having a density of 76° to 78° Tralles. This, upon repeated fractioning over caustic lime, affords a distillate boiling at 88–89°, possessing the odor of allyl alcohol. The low boiling point is due, however, as analysis showed, to the presence of a molecule of water, which could not be removed by fractional distillation. When dehydrated by chemical means, the boiling point rose to 96–97°, which is that of the pure allyl alcohol. The identity of the product was further established by direct conversion into bibromhydrin, boiling at 217° and into allyl iodide, boiling at 101°.—*Ber. Berl. Chem. Ges.*, vii, 1381, Nov., 1874. G. F. B.

5. *On the Gases evolved from Apples.*—BENDER has made an examination of the gases evolved from ripe apples. The fruit cut in pieces was placed in water free from air contained in a flask furnished with a delivery tube. On raising the temperature to 60°, the gas began to come off, and at 100°, the evolution was rapid. Four moderately sized apples afforded about 100 c.c. of gas. Upon eudiometric analysis this gas—great care having been taken to exclude the air—was proved to consist of 59·37 per cent of nitrogen, and 40·20 per cent of carbonic acid, the remaining 0·43 per cent being oxygen. In a subsequent experiment, 31·07 per cent of the gas was carbonic acid and 68·93 per cent of nitrogen. The author believes that the carbonic acid comes from a fermentation within the fruit, the ferment being produced at the time of ripening.—*Ber. Berl. Chem. Ges.*, viii, 112, Feb., 1875. G. F. B.

6. *On the Influence of Light upon Cane Sugar.*—KREUSLER has made a series of experiments to test the assertion of Raoult that pure cane sugar in aqueous solution, both air and ferments being completely absent, underwent an inversion solely through the influence of light. While Raoult used only a solution containing 10 grams of white sugar to 50 grams of water, Kreusler employed solutions of various strengths. These were sealed up in glass tubes, after careful exhaustion, and with the exception of one, were exposed, in a window facing the southeast, to the direct sunlight for eleven months; more than twice the time given by Raoult. On opening the tubes, they still showed a good vacuum, and the liquid was clear and free from microscopic vegetation of any sort. But when tested with Fehling's solution, neither the liquid exposed to the light nor that preserved in darkness gave the slightest reaction. Upon repeating the experiment, however, taking care to introduce a small quantity of air into the tubes before sealing, organic ferments were found present, and the liquid reduced strongly the copper test; that exposed to light being the more active. The author believes therefore that an imperfect exhaustion in Raoult's tubes was the cause of the results he describes. —*Ber. Berl. Chem. Ges.*, viii, 93, Feb., 1875. G. F. B.

7. *Reflecting Lever.*—M. CORNU has devised an extremely simple but ingenious substitute for the spherometer which he calls the reflecting lever, or reflecting spherometer. Suppose a short lever like the beam of a balance supported on four points, one at each end, and two replacing its central knife-edge; also that a vertical plane mirror is attached above it in a plane perpendicular to its length. If now the four points lie in one plane and a thin plate is placed under the two central ones, the lever will be in unstable equilibrium, and will rock from side to side. Viewing with a telescope the reflection of a distant scale, the thickness of the plate is readily deduced from the angular motion. For, calling i the inclination of the beam, p the thickness of the plate, and l the half length of the lever, we have $\sin i = \frac{p}{l}$, or if i is small, $p = il$. The motion of the beam is double this, and the reflected ray travels twice as rapidly as the mirror; hence calling n and n' the two readings, and D the distance of the scale, we have $p = il = \frac{n-n'}{4D} l$. If the four points do not lie in one plane a correction is readily applied. Comparing the results, he concludes that while this instrument is very easily made and absolute measures readily obtained within a tenth of a per cent, it compares favorably, even in accuracy, with the best spherometers.

This instrument is also readily applicable to the measurement of curvatures and for many other purposes where a minute change of position is to be noted.—*Journ. de Phys.*, iv, 7. E. C. P.

8. *Maps of the Solar Spectrum.*—Mr. LOCKYER laid before the Royal Society, at a recent meeting, a portion of a new map of the solar spectrum. The scale is four times that of Angström and includes the portion between the wave-lengths 39 and 41. A photographic print of the spectrum was employed, and from it curves were constructed by which the wave-lengths of the new lines were obtained graphically. In Angström's map (spectre-normal) 39 lines are given within these limits; Angström and Thalen's map, 185; Cornu's new map, 205; and in the present map, 518.

The spectra of the following metals are also given: Fe, Co, Ni, Mn, Ce, U, Cr, Ba, Sr, Ca, K, Al, representing 416 lines.—*Nature*, 238. E. C. P.

9. Mr. J. H. N. HENNESSEY has also prepared a map of the atmospheric lines drawn on the same scale as that of Kirchhoff. The observations were made at an elevated point, and the spectrum, when the sun was high, compared with that at sunset. Observations were also made to ascertain whether any of the lines which appeared, when the sun was low, were due, not to specific atmospheric absorption but to the general weakening of the light. The intensity of the spectrum was reduced in various ways, but the best results were obtained by observing the sun through a haze, with the result that none of the additional lines appeared to have any other origin than elective atmospheric absorption.—*Nature*, 278. E. C. P.

10. M. CORNU has prepared a map of the ultra-violet portions of the spectrum, to form a continuation of that of Angström and Thalen. It includes, in common with their map, the portion from *h* to *H*, but contains about four times as many lines. Compared with the plate of Mascart, it has the double advantage of being a normal map, that is, the position of the rays are proportional to their wave-length, and that the general appearance of the lines is represented more faithfully. Three series of photographs were taken with a ruled plate of Nobert, a flint-glass prism and a prism of Iceland spar, using the ordinary ray. From the first of these, Cornu determined the wave-lengths of 36 of the more marked lines which serve as a basis for the rest. The prismatic spectra are more distinct and of greater extent, and from them he has deduced over six hundred additional rays between *N* and *O*, or between the wave-lengths of 4100 and 3637. The agreement of the principal lines with the measurements of Mascart is complete.—*Bib. Univ.*, ccv, 62. E. C. P.

11. *Index of Refraction of thin plates.*—M. WIEDEMANN describes a new method of determining indices of refraction applicable in certain cases where the ordinary methods fail. The light of a gas-burner passes through the slit of a collimator and is rendered parallel. It then passes through a tank formed of plates of glass cemented together, and filled with oil of cassia. In this tank is immersed the plate of glass to be tested. It is fixed to the axis of a theodolite, whose limb serves to show the angles through which it is turned. The light is then dispersed by a prism and the spectrum thus formed is observed by a telescope. As oil of cassia has an index much greater than that of most kinds of glass, there may be produced a total reflection at the surface separating the oil and glass. Then on turning the plate, at first normal to the light, either to the right or left, a gradual extinction of the spectrum is effected, commencing with the more refrangible rays. The spectrum is extinguished as if a black curtain was drawn over it, the edge indicating the limit of the total reflection. Calling a the angle between the two symmetrical positions of the glass corresponding to the limit of extinction of a particular ray, $\sin \frac{1}{2}a$ will be the index of refraction of the oil with regard to the glass. Multiplying this by the index of the oil gives the index of the glass. The thinner the plates the more exact are the results given by this method. The index of refraction of liquids may be determined by a similar method. Two plates of glass are cemented together, leaving between them a thin layer of air. When this is too thin the limit of total reflection is not clearly marked, because a portion of the light penetrates the layer of air and enters the second plate. If, on the contrary, the layer is too thick, the limit of total reflection also loses its clearness, in consequence of the numerous interference bands. By interposing sheets of mica, whose thickness is gradually reduced, the best thickness is readily obtained. On turning the plates so that the angles of incidence become smaller and smaller, starting from a position a little beyond that of total reflec-

tion, interference rays are observed which pass over the spectrum from the red toward the blue. The angles of incidence having become smaller than the limiting angle, the direction of the displacement of the rays alters, and they move from the blue toward the red. These last are the interference rays ordinarily produced by thin films.—*Bib. Univ.*, cciv, 340. E. C. P.

II. Geology and Natural History.

1. *The Elevation of certain Datum-points on the Great Lakes and Rivers and in the Rocky Mountains;* by Jas. T. Gardner, Geographer. Washington, 1875. (Annual Rep. U. S. Geol. and Geogr. Survey of the Territories, for 1873, pp. 629–659.)—As Geographer in the Rocky Mountain Expedition under the charge of Dr. F. V. Hayden, Mr. Gardner found it necessary to fix upon some datum-point to serve as a base for the reckoning of altitudes, and met with a first difficulty in the different altitudes assigned to Denver, Colorado, they diverging between 200 and 300 feet. To eliminate the error, he undertook the "reconstructing of all possible lines of levels from the ocean to the Rocky Mountains, using only official reports by engineers, and checking them by personal examinations of their note-books and working profiles whenever practicable." The chapter here published contains the results of his long labor. Difficulty was found in the fact that railroad surveys from the Atlantic border had not started from the same stage of tide level, in errors from careless computation, and from other causes. The final results have proved that the elevation of the Great Lakes and the surrounding country is about *nine feet* more than previously reported by the State Geologist of Ohio; that St. Louis is 23 feet higher than made by Humphreys and Abbott; that Kansas City and the surrounding country for many hundred miles south and west is more than 100 feet higher than heretofore reported; Omaha 31 feet higher; Indianapolis about 100 feet. The following are a few of the levels ascertained:

Mean level of Lake Ontario above mean tide level	249·99 ft.
" Lake Erie	573·08
" Lake Huron	589·99
" Lake Michigan	589·15
Low water in Ohio at Cincinnati	440
Cairo city base, ordinary low water	291·23
Saint Louis directrix	429·29
Omaha, low water base of U. P. R. R.	977·90
" Depot grounds	1,060·40
Denver, Col., O. P. & K. P. R. R. passenger depot	5,196·58
Cheyenne, U. P. passenger depot	6,075·28
Golden, Colorado	5,728·98
Ogden, Utah, depot track	4,303·30
Pike's Peak	14,146·68

The level of mean tide at Albany, N. Y., above mean tide at New York City, was taken at 4·84 feet, as ascertained by the Coast

Survey. The mean height of Lake Erie was arrived at by means of levelings (1) on the Erie Canal from Albany to Buffalo; (2) from Philadelphia by railroad through Harrisburg, Pittsburg, Crestline to Cleveland; (3) from Albany by railroad to Buffalo; (4) by the North Central Pennsylvania R. R.; (5) by the Erie R. R. to Dunkirk and Cleveland; (6) by the Canadian Grand Trunk R. R. The results obtained by these six routes respectively are the following: 573·08, 572·037, 572·670, 570·750, [581·20], 571·67 feet. This example illustrates the method used for obtaining the true elevation, and shows also the high degree of confidence that may be placed in them; and further that Mr. Gardner makes in his memoir a contribution to North American topography of extreme importance.

A few others of the heights ascertained are: Quebec, mean tide level, 15·37 feet; Montreal, summer water-level, 30 feet; Lake Champlain, mean level at Whitehall, 100·84; Pittsburg, Pa., low water in river, 699·20; Louisville, Ky., low water above Falls, about 404; New Albany, Ind., low water in 1857, 379·75, and the depot of L. N. A. & C. R. R., 451·75; Rock Island, Ill., high water in Mississippi in 1852, 566·68; Terre Haute, Ind., high water in Wabash, 485·55, and ordinary water, 467·45; Mount Lincoln, Colorado, 14,296·66 feet. J. D. D.

2. *Geological Survey of Canada*, Alfred R. C. Selwyn, Director. *Report of Progress for the year* 1873–4. 268 pp. 8vo. Montreal, 1874. (Dawson Brothers.)—No annual Geological Reports on this side of the Atlantic exceed in value those of the Canada Geological Survey. The volume recently issued contains, besides an introductory report of 16 pages by Mr. Selwyn, observations in the Northwest Territory, from Fort Garry to Rocky Mountain House, by the same; Report on the country between Red River and the South Saskatchewan, with notes on the Geology of the region between Lake Superior and Red River, by Mr. R. Bell; Report on Geological explorations in British Columbia, by Mr. James Richardson; Report on explorations and surveys in Frontenac, Leeds and Lanark Cos., with notes on the graphite of Buckingham and apatite of Templeton and Portland townships, Ottawa Co., by Mr. H. G. Venner; Reports on the survey of the Coal Field of Cumberland Co., Nova Scotia, by Mr. S. Barlow and Mr. W. McQuat; on explorations and surveys in Cape Breton, Nova Scotia, by Mr. C. Robb; on the iron ores of Canada and their development, by Dr. B. J. Harrington; and notes on the Cretaceous fossils collected at Vancouver and the adjacent islands, from the coal series, by Mr. J. F. Whiteaves. A Report by Mr. Billings on fossils from Gaspé, and another by Mr. Weston are in progress; and also a Report by Prof. Bailey and Mr. Ells, on New Brunswick Geology.

Mr. Selwyn states, in his Introduction, that the facts gathered tend to prove that *lithology* among crystalline rocks is not a safe test of geological age, observing that this conclusion is favored by the recent researches of Mr. Richardson in British Columbia, which

show that "epidotic, chloritic and serpentinous rocks, with crystalline limestones and magnetites, are as characteristic of Upper Paleozoic, and perhaps also of even later formations, where they have been subjected to an equal amount of plication and folding, as they are of the oldest Paleozoic and protozoic strata, such as those of Eastern Canada and the New England States." He observes that Sir William Logan has been carrying forward extended and thorough investigations in Canada on this point, connected with a study of the true stratigraphical relations of the rocks, and that he will give a full report as soon as "the necessary examinations have been completed."

The Report also states that *boring for water and coal* in the Northwest Territory with a diamond-pointed drill was undertaken the past year, but that there was not time after receiving the apparatus for obtaining any important results. The boring will be resumed this season.

In his Report on the Northwest Territory, Mr. Selwyn states that, going westward from Fort Garry (or Winnipeg), the capital of Manitoba, lat. 49° 52′ N., long. 96° 50′ W., *only drift*—sand, clay and gravel—was seen along their course for a distance of 885½ miles. The first terrace plain in that direction has an average breadth of 120 miles, is 75 to 100 feet above Lake Winnipeg, and but 700 or 800 feet above the sea level. The drift of its surface is apparently underlaid at no great depth by Silurian and Devonian strata. Its western limit is marked by a range of low hills extending from Pembina Mountains by the Riding, Duck and Porcupine Mountains, to the Basquia Hills. Here there is a rise to a *second* prairie level, which is undulating or hilly and "attains an average elevation of 1600 feet." A *third* prairie level commences at the Thickwood Hills, 20 miles west of Carleton; it is 1900 to 2000 feet above the sea; the limestone boulders on it do not reach farther west than the longitude of Fort Pitt. Sand is the material of the deposits, and it is mostly unstratified, becoming imperfectly stratified and calcareous to the westward. At Fort Ellice, the valley of the Assiniboine (as stated by Dr. Hector) is 240 feet deep: the upper 100 feet consist of this drift deposit, and the rest of Cretaceous beds.

The watershed between the Qu' Apelle to the southwest and the Saskatchewan and Assiniboine to the northwest, is mostly a great salt plain, with many pit-holes and some large salt lakes.

On the North Saskatchewan River, between Edmonton and Rocky Mountain House (138 miles), several horizontal *coal beds* were observed, one of them 18 to 20 feet thick, and favorable for working. The coal is hard and bright, jet-like, and much superior to that found farther south along the boundary line and in the Qu'Appelle valley. The age of the latter is pronounced probably Tertiary; of the former, it is stated to be yet uncertain. The plants in the two are the same. The coal-field is stated to extend at least to the Athabasca River on the north, and Red Deer River on the south, and to have an area of 25,000 square miles. Analyses of the coals by Dr. B. J. Harrington are given on pages 63, 64.

Mr. Bell states that the valley of Red River from Fort Garry to Lake Winnipeg is underlaid by Lower Silurian magnesian limestone, and that above are Devonian rocks, which are exposed on various points and islands in Lakes Manitoba and Winnipegoos. Coal was found among sandy and clayey strata in the Dirt Hills, 96 miles southwest of Fort Qu'Appelle, and at the Woody Mountains in the intermediate region. At the latter place, where the formation is 200 feet thick, eight seams were counted, one of them eight and another five feet thick. Mr. Bell, adopting the view that the coal-bearing strata above the *Inoceramus* shales and sandstones of the Souris are probably Tertiary, states that the line between the Tertiary and the Cretaceous formation enters British territory in the neighborhood of the Roche Percè, and thence runs northwestward to the Elbow of the South Saskatchewan, passing about 40 miles north of the Dirt Hills. The greater part of the area between the two branches of the Saskatchewan is occupied by the "coal-bearing Tertiary." The coal beds are partly true lignite.

With regard to the *Drift*, Mr. Bell states that *the direction of the striæ* to the north and northwest of Lake Superior is in general *southwestward*, and that the same course prevails as far east as the Ottawa River; and "there is little doubt that the same course prevails for some distance to the north and west of Lake Winnipeg; for the plains west of the lake and of West River are covered with the debris of rocks derived from the east of the lake."

These observations on the scratches northeast of Lake Huron confirm the statements of others, and sustain the conclusion of the writer therefrom*—that the greater height of the ice-surface in Canada was over the watershed between the St. Lawrence and Hudson Bay. The facts from the vicinity of Lake Winnipeg show further that a line of *greatest height* was continued from this region northward or northwestward. It does not follow that the height along this line to the northward was as great as over the watershed; but only that the greatest height for those latitudes was there, or that it was great enough to determine a southwestward movement in the ice. The greatest height would have been where there was the greatest amount of precipitation, supposing the melting the same; and this, as the writer observes in the same article,† would have been toward the southern and coastward limit of the glacier. Over the interior of the continent, both the small amount of precipitation and the high degree of heat in summer would have put the southern limit of the ice north of the limits of the United States. The summer isotherm of 72° F., which passes over southern Ohio—the south limit there of the drift,—extends in the interior, about the meridians 98° to 105°, northward to latitude 47°; and over this area and also its extension far north and likewise far west to a line passing by Sierra Nevada and the Dalles in Oregon, the amount of annual precipitation, according to Schott's Rain Chart, is *sixteen inches and less.*

* This Journal, III, ii, 324; v, 204. † Ibid, v, 206, 208.

Mr. Harrington's report on the rocks and minerals is an important one. It contains analyses of oligoclase, hornblende, magnetites, hematites, limonites and other species, besides observations on the associations of the species. J. D. D.

3. *An Essay concerning important Physical Features exhibited in the Valley of the Minnesota River and upon their signification;* by G. K. WARREN, Major of Engineers and Bvt. Maj. General, U. S. A. 22 pp., 8vo., with several maps. Engineer Dept., U. S. Army. —Major Warren draws attention, in this Report, to the width and extent of the valley of Minnesota River (one of the head tributaries of the Mississippi) a feature continued even to its source in Lake Traverse, and to the great disproportion between its size and that of the stream now occupying it. He next states facts, derived from Prof. H. Y. Hind and other observers, respecting the great extent of the level region and stratified drift deposits about Lake Winnipeg, and the uninterrupted continuation of the deposits to Lake Traverse. The conclusion deduced is that in the Glacial era the land to the north about Lake Winnipeg was more elevated than now, so that the drainage from that lake was to the south, instead of, as now, through Nelson's River, to Hudson Bay; and that Lake Traverse was really the south end of Lake Winnipeg, and Minnesota River the outlet by which the great flood of waters passed south to the Mississippi. The Mississippi River would then have had its head waters in the streams that enter Lake Winnipeg from the west and north.

The highest part of the region about Lake Traverse is 995 feet above the sea, while that of Lake Winnipeg is about 630; so that a rise of but 365 feet to the north would produce the change of course. He observes that the height of the lake terrace above the ancient bed west of Pembina, as given by Mr. Owen, is 210 feet; that the lake bed there is about 50 feet above Red River, and this river at this point is about 100 feet above Lake Winnipeg; and hence he adds, "this lake terrace is 360 feet above the level of Lake Winnipeg—a height sufficient, as I have shown already, to extend the present Lake Winnipeg to Lake Traverse." Professor Hind, as quoted, says that "the Pembina Mountain [but 210 feet high] is *par excellence* the ancient beach in the valley of Lake Winnipeg. Dr. Owen thus describes it as it presents itself a few miles south of the 49th parallel."

When the southward slope of the surface came to its end through a northern subsidence, Nelson's River became the outlet —a narrow gorge abounding in water-falls. J. D. D.

4. *An examination of the Theories that have been proposed to account for the Climate of the Glacial Period;* by THOMAS BELT, F.G.S. (Quart. J. Sci., Oct., 1874.)—After a discussion of the merits of various theories for accounting for the cold climate of the Glacial period, Mr. Belt argues strongly for the old theory of a change in the obliquity of the ecliptic. He admits that the decision of astronomers is against it, but he holds that "until astronomers have reconsidered this question with the light of our

present knowledge of the figure of the earth, geologists should not be prevented from speculating on the probability of great changes in the obliquity of the ecliptic having caused former great variations of temperature." Mr. Belt also touches on the lowering of the sea-level by the accumulation of ice over the land, and says that, on the supposition that the ice was equal in the two hemispheres at the same time (the view he sustains), the lowering of the sea-level "could not have been less than 2000 feet, and may have been much more."

Since any lowering of the water line of the ocean by the abstraction of water to make the Glacial ice would have been alike on all the coasts of the world, Mr. Belt's estimate of the amount in the Glacial period is open every where to demonstration. On the Atlantic border of North America, such a change—which is equivalent to a rise of the land of 2000 feet—would have led to an immense amount of valley-excavation along the many rivers; and such valleys, so recently made, should still intersect the alluvial, Tertiary, and Cretaceous deposits of the very wide coast region of the continent, or be but partially obliterated. Instead of this, this coast region is remarkably flat and even and is intersected only by narrow river channels. There is nothing comparable in any part to the fiords of Glacial latitudes; and, more than this, there is little evidence of recent excavation very much below the present sea-level. The same fact is true of other continents.

A source of cold not considered by Mr. Belt is suggested in the writer's Geology (new edition), viz: that there was *an elevation of Arctic lands sufficient to exclude the Gulf Stream from the Arctic regions.* If these tropical waters—which now distribute over the northern latitudes, according to Mr. Croll, 154,959,300,000,000,000,000 foot-pounds of energy in the form of heat per day—were thus prevented from making their circuit over the polar regions north of the continents, the flow northward by Iceland would be stopped, and, owing to the northeastward trend of the American coast from Florida to the southeastern cape of Newfoundland, the Gulf stream would be mostly confined to a circuit within the tropical and warm-temperate zones.

This cause, in addition to the direct cold-producing effect of an increased amount of Arctic land, would thus have been sufficient to produce all the lowering of temperature needed for a Glacial period. And it would have made an epoch of cold in any period of the globe. Moreover, the deep submergence of Behring's Strait (now but 180 feet in depth), and of the region around, by letting in the Pacific tropical current, would at any time, as in the Miocene period, have made a warm Arctic. This explanation of the warm Arctic climate in the Miocene, which is presented in the Geology,* was suggested to the writer, as there stated, by Professor F. H. Bradley; and the other respecting Glacial climate may be considered a corollary from it.

Professor A. E. Verrill has recently called the attention of the writer to another effect from this confining of the Gulf Stream to

* Manual of Geology, 2d edit., p. 541.

the Atlantic: that the heat which is now carried to the north would then be kept mainly within the warm temperate and tropical latitudes of the ocean, and that, consequently, the other requisite of a Glacial era, *very abundant precipitation, would have existed concurrently with that of unusual cold over the land*—conditions, as Professor Verrill states, that would make a storm region of tremendous effects over the ice within 300 or 400 miles of the warm sea limit. All the conditions demanded are therefore met by this theory.

The high-latitude changes of level here appealed to may have produced effects at intervals through all geological time, though of feeble intensity in the Paleozoic. Agassiz, in 1840, in his *Études sur les Glaciers*,—a work that made an epoch in geology by its demonstrations of the glacier origin of the Drift,—suggested also that the principal epochs of life-extermination in geology were epochs of cold—"que les plus grands froids ont terminé chaque epoque géologique,"—cold being a cause of extermination; and there is reason to believe that he was right as to some at least of the epochs. The writer recognizes two such in his Geology, one at the close of the Carboniferous, and another at the close of the Cretaceous (the latter in the 1st edition of the work); and he alludes to the subject here in order to give the credit of first bringing forward the principle to Agassiz. "The indications of floating ice which Ramsay has found in the British Lower Permian may be a mark of the slow approach of such an era of cold"* at the close of Paleozoic time; and the Triassic rocks of eastern North America seem to bear, in their large rounded stones and the nature of the beds, evidence that the excessive precipitation and also the cold of such an epoch had then not yet passed.† J. D. D.

5. *On the Submergence during the Glacial period.*—Professor Croll, in the Geological Magazine for July and August, 1874, presents the following as the cause of the apparent subsidence, or rise in the water level, which took place before the disappearance of the ice of the Glacial period; or, as he has it, the submergence during the Glacial period, he holding that the whole period was a period of lower level of the land than now. He proceeds on the supposition, before argued for by him, that the cold of the Glacial period was due to the eccentricity of the earth's orbit, and that it affected the northern and southern hemispheres alternately.

The removal of two miles of ice from the Antarctic Continent would displace the center of gravity 190 feet, and the formation of a mass of ice of one-half this thickness in the Arctic regions would carry the center of gravity 95 feet farther in the same direction. Further, the area of the Antarctic ice-cap being $\frac{23}{46}$ of that of the ocean, therefore, allowing 0·92 for the density of ice, the melting of $25\frac{1}{2}$ feet of ice from the cap would raise the general level of the

* Manual of Geology, 2d edit., p. 431.

† The agency of ice in the deposition of the Trias of Eastern North America has been suggested by T. A. Conrad, and also by H. Wurtz, and by both in 1869.

ocean one foot; and a mile of ice, 200 feet. The other mile of ice melted is balanced by the abstraction of water to add a mile of ice to the Arctic. These numbers added, says Mr. Croll, give 485 feet for the amount of submergence that would be thus occasioned at the North Pole, and 434 feet for that in the latitude of Edinburgh; or if the ice had twice the supposed thickness, the amount of submergence would have been twice this amount.

The facts from Eastern North America do not appear to favor Mr. Croll's conclusion. The amount of subsidence indicated by raised shell-beds on the St. Lawrence at Montreal, near latitude 45°, was about 500 feet; on the coast of Maine, near latitude 44°, only 200 feet, when it ought by the theory to have been but a few feet less than at Montreal; and along the southern shore of New England, near latitude 41°, only 50 to 100 feet, the larger of which numbers is much less than a fourth of what the theory requires. It seems hence to follow that the curve of submergence did not correspond with the curve which would have been produced in the ocean's surface by a change of the center of gravity.

The Coral Island subsidence over the Pacific, which affected a very large portion of the tropical part of that ocean, must have been in progress through the Glacial period, and the amount of it was certainly several thousand feet. Here was a great change of level *in the tropics*, which no change in the earth's center of gravity could have produced. It was in a sense local, the continental lands adjoining not sinking with the ocean's bottom, nor all parts of the ocean alike. It is evidence that the cause of the great change of water level during the Quaternary age, as well as in earlier time, resides in the crust itself. The facts from the shell beds of the St. Lawrence and the coasts of New England appear to be equally evidence of a *bona fide* subsidence, which no cause affecting the ocean as a whole will account for. J. D. D.

6. *Recent Changes of Level on the Coast of Maine, with reference to their origin and relation to other similar changes;* by N. S. SHALER. (20 pp., 4to, 1874, from the Mem. Bost. Soc. Nat. Hist., ii, 322.)—The evidence of the changes of level here discussed was gathered from the Quaternary deposits of Maine and of the coast south. Professor Shaler concludes that the evidence sustains the view that there was a subsidence or "depression" of the land increasing in amount, though somewhat irregularly, to the northward, after "the first division of the Glacial period;" and that subsequently there was "a return of the ice in the shape of a set of local glaciers which covered the shore at Mt. Desert, and along most of the territory at least as far as New Brunswick, and persisting until the final re-elevation of the land to near its present level."

The cause of the depression is discussed in the closing pages. He remarks that the only hypothesis as yet advanced to account for it is one proposed by Adhémar; overlooking the fact that the writer, first in 1856,* offered another, referring the great Quater-

* Amer. Jour. Sci., II, xxii, 346. The same conclusion is involved in his earlier paper in 1846, 1847.

nary movements of the crust,—the *upward* for the Glacial period, the *downward* for the Champlain or Fluvial period, and then a succeeding *upward* rise—like the other great movements, to the lateral pressure in the earth's crust due to contraction from cooling.

Adhémar's hypothesis, as explained by Mr. Shaler, supposes a displacement of the center of gravity in consequence of the unequal accumulation of ice at the poles, much like that recently proposed by Mr. Croll. Professor Shaler rejects this explanation, chiefly because the Glacial phenomena of the two hemispheres have not yet been proved to be alternate, and because the amount of depression does not increase with regularity over the whole northern hemisphere; and then presents his own hypothesis, which attributes the depression to *the weight of the ice-mass over the land.* After speaking of the continents as having their positions determined by "constant tensions" (meaning, perhaps, to include *lateral pressure*, which far exceeds tensions in importance) and the weight of the mass, he says, "it seems evident enough that we may more reasonably look to the weight of the ice accumulated on the continents during the Glacial period for the depression of the land-areas it occupied, than to any other cause."

This theory assumes that a cap of ice "a mile or more thick," equivalent, as the author states, to half this thickness in "ordinary rock," depressed the earth's crust 500 feet and upward over the area from the St. Lawrence and great lakes to the Arctic, notwithstanding that the arched crust had a thickness of probably 100 miles. There is no doubt that such a weight—equal to a cap of rock, say one hundredth as thick as the earth's crust—would tend to cause a depression, for this tendency exists even under any small weight. But, considering the form and nature of the crust, and the lateral or tangential pressure existing throughout it, shown by Mallet to be enormous, it seems to be far from probable that mere weight can account for so great a movement. Lateral pressure has been the chief agent in oscillations of level and mountain-making over the earth; since Archæan time, making (1) the Green Mountains, (2) ridges in Nova Scotia and New Brunswick, and (3) the Alleghanies, on the *eastern* border of the North American Continent, at long intervals; and *subsequently*, after other long intervals, raising (1) the lofty Wahsatch Mountains and Sierra Nevada, (2) ranges of Cretaceous mountains, and (3) Tertiary ridges, on the western side of the Continent; besides (4) elevating, during the Cenozoic, the great Rocky Mountain mass 8,000 to 10,000 feet, —an effect that no weighting was employed to produce. And even in the Quaternary, though perhaps the work was begun before, there was the great subsidence over the tropical Pacific—the coral island subsidence (and another similar, probably over the tropical Atlantic), which the weighting of the islands by coral growths was certainly inadequate to produce, and which is naturally another consequence of the uneasiness of the crust owing to lateral pressure.

Here are vast results — elevations and subsidences — accomplished by lateral pressure. Now, that elevation of the land over the higher latitudes which brought on the Glacial era, is a natural result of the same agency, and a natural, and almost a necessary, counterpart of the coral island subsidence, which must have been then in progress. The accumulating, folding, solidification and crystallization of rocks attending all the rock-making and mountain-making through the Paleozoic, Mesozoic and Cenozoic eras, had greatly stiffened the crust in these parts, and hence, in after time, the continental movements resulting from the lateral pressure necessarily appeared over the more northern portion of the continent, where the accumulations and other changes had been relatively small. To the subsidence which followed the elevation the ice-cap may have contributed in some small degree. But the great balancing movements of the crust of the continental and oceanic areas then going forward must have had a greatly preponderating cause in the oscillating agency of all time, lateral pressure within the crust. J. D. D.

7. *Age of the Lignitic Coal Formation of Vancouver Island.* Letter to the editors, from Alfred R. C. Selwyn, F.R.S., Director of the Geological Survey of Canada, dated March 3d.—I wish to record my dissent from the statement made by Professor Lesquereux, page 365 of Dr. Hayden's Report on the U. S. Geological and Geographical Survey of Colorado, 1873, to the effect that the coal of Nanaimo, Vancouver Island, is referable to the lower American Eocene.

Careful surveys have now been made by the Canadian Geological Survey of the Nanaimo coal basin, and it is proved beyond the possibility of a doubt that the coal beds there are overlaid by a succession of strata, shales, sandstones and conglomerates, having a thickness of nearly 4000 feet, and holding from base to summit marine Cretaceous fossils, *Ammonites*, *Baculites*, *Inocerami*, and others.

Maps and sections showing the relative position of these beds and of the coal seams are given in the Report of the Geological Survey of Canada, 1872–'73; and I would beg to refer Professor Lesquereux to them for information concerning the coal rocks of Vancouver Island. As he makes no reference to the report named, I conclude he has not seen it.*

8. *Note on the genus Opisthoptera* Meek, 1872, *and Anomalodonta* Miller, 1874. (Communicated.)—Mr. S. A. Miller, in a reply to my note on the above mentioned genus,† published in the September number (1874) of the Cincinnati Journal of Science, endeavors to defend his substitution of the name *Anomalodonta* for Meek's earlier name. This he does on the ground (1) that *Megaptera*, having been previously used for a genus of whales, could not stand; (2) that although Mr. Meek had subsequently

* The facts are briefly mentioned from the Canada Geological Report for 1872–1873 in vol. vii of this Journal, pp. 517, 518, 1874.—Eds.

† See this Journal, viii, 218, 1874.

(but previous to the publication of Mr. Miller's name) proposed to substitute for it the name *Opisthoptera*, he did it only *provisionally*, and had not himself adopted it in the subsequently published Ohio report; and (3) that neither Meek and Worthen jointly, nor Mr. Meek alone, had fully defined the generic characters of their type.

No further notice would be taken of the matter, were it not that silence might be construed into acquiescence in views and practices that it is the interest of all working naturalists to discountenance. In the first place, it is by no means a *settled* question among naturalists that the same name may not be used for genera in different sub-kingdoms, or even in different classes of the same sub-kingdom. This is the practice of some of the best and most renowned naturalists. *Troglodytes* is constantly used for a genus of apes, and also for a genus of birds, and many other cases might be mentioned. I think, however, with Mr. Meek, that it would be better to change the latter name, but he seems to have deferred to the high authority mentioned in only provisionally proposing to change it in the case of *Opisthoptera*. That he did not himself formally adopt the latter name in the Ohio Report has no bearing on the question. By repeating there the previously published note on *Opisthoptera*, he manifestly repeats the proposal to substitute the latter name on precisely the same ground that he did at first, namely, in case it should be found generically distinct from *Ambonychia* and the name *Megaptera* should be objected to. But it would not have altered the case if he had there said nothing about it, or even if he had proposed to retract both *Megaptera* and *Opisthoptera* entirely. As soon as the name was published it became the property of science, and he had no more right to dispose of it than any other person.

Mr. Miller's argument that both *Megaptera* and *Opisthoptera* should be discarded because *full* generic descriptions of them were not published, is not, and cannot be, sustained by general usage. Even in recent zoology, where it is possible to ascertain clearly all the characters, no such rule is generally followed. Such a rule would be utterly inadmissible in the department of fossil shells, because of the exceeding rarity of specimens that are even approximately perfect; for even in the best specimens the most important features (as, for example, the internal markings and teeth in bivalves) are generally obscured. Consequently, many of the genera of the latter kind of fossil shells have been proposed mainly or entirely upon external characters. The rule would endanger the name *Anomalodonta*, because nothing is yet known of the pallial line and pedal muscular scars of that shell, to say nothing about the extraordinary position of the adductor impression in Mr. Miller's figure. Hundreds of cases might be mentioned of genera established upon external characters.

Unlike some other genera, *Opisthoptera* has its more conspicuous characters external, namely, its form and surface ornamentation, which when taken together are quite sufficient to distinguish

it from related types, even from the broad types of *Myalina*, from which the hinge characters shown by Mr. Miller would not separate it. As Meek and Worthen, and still later, Mr. Meek alone, have fully described and illustrated these external characters of their type, they have given sufficient means for its identification, which even Mr. Miller found no difficulty in doing. To Mr. Miller's other remarks it is hardly necessary to reply.

As to the affinities of *Ambonychia* with the *Aviculidæ*, which Mr. Miller refuses to admit, on account of the equality of the valves, I need only to remark that to make such family distinctions as that proposition, would require us to divide well defined genera, some species of which would fall in one family and some in another, the genus *Inoceramus* being an example. The true (Triassic typical) *Monotis*, *Halobia*, and others are included by the highest authorities among the Aviculidæ, and yet they are equivalve. He refers to McCoy as the first to refer *Ambonychia* to the Aviculidæ, being evidently unaware of the fact that in 1833 Goldfuss in his great work, *Petrif. Germ.*, did the same. Conrad, Woodward, Brown, Dr. Carpenter, and Dr. Stoliczska all have done the same, the latter even placing it near *Avicula* in the section Aviculinæ.

Returning to *Opisthoptera*, I will state its synonymy thus:

Genus OPISTHOPTERA Meek. *Megaptera* Meek and Worthen, 1866 —not *Megaptera* Gray, 18—. *Opisthoptera* Meek, 1872. *Anomalodonta* Miller, 1874. C. A. W.

9. *Costa Rica Geology.*—Mr. W. M. Gabb, in a letter dated San José, Costa Rica, February 1st, corrects his former calculation of the height of Pico Blanco, stated on page 199 of this volume, making it 11,877·8 feet. The error came from a slip in the use of a formula.

Mr. Gabb also states that on the 26th of January he ascended the volcanic peak Irazu, and obtained barometrically for its height 11,356·5 feet. The volcano is extinct, excepting a little smell of sulphur in the crater. He further remarks that the peak of Turrialba, nine or ten miles from Irazu, is so nearly equal to the latter in height that much difference of opinion exists as to which is the highest. This volcano "sends out a constant small cloud of vapor."

10. *The Gulf of Mexico in the Miocene.*—Prof. Hilgard has suggested, in view of the absence of marine Miocene beds over the Eocene of Louisiana, Mississippi and Alabama, and the presence there of the beds of the Grand Gulf group without remains of marine life, that the Mexican Gulf was fresh or only brackish in the Miocene, and made so by the elevation of a barrier across from Yucatan.* Mr. W. M. Gabb, in letters to the writer, states that his observations among the West India Islands lead him to believe, in view of the distribution of marine Miocene beds, that "in the Miocene period, Cuba, like Jamaica and Santo Domingo, was much smaller than at present, and may even have

* This Journal, II, ii, 397.

been only a string of small islands; while Yucatan, the Bahamas, Bermudas and much of Florida did not yet exist, and their surface beds had not yet been deposited."

He remarks: "The additional facts I have acquired in Costa Rica geology go strongly to sustain the opinion I have already advanced, that no sedimentary rock older than the Miocene is found here. The Carboniferous formation is Miocene; and wherever it is unaltered it contains coal-beds, some of which are two yards thick. Now I have seen this coal, as the green streaks show, all the way around from near San José to the end of Talamanca, and we know that heavy beds occur in Chiriqui *on the Atlantic side*. And all along the coast, as at Boca Brava, Punta Uvita, Pirris, Tarcoles, etc., coal is found. The existence of these Miocene beds on the Atlantic side proves that the gulf was occupied by marine waters as now. J. D. D.

11. *On the Earthquakes of Southern Italy;* by EDWARD SUESS. 32 pp., 4to, with three plates. Vienna, 1874. (From vol. xxxiv, Denkschriften der Kaiserlichen Akademie der Wissenschaften.)—Professor Suess first describes with some detail the geological structure of Sicily, and of the neighboring districts of Italy, to the knowledge of which he has contributed so much by his personal labors. It is shown that in this region there is, in the crystalline rocks, a zone of granite and gneiss, and a bordering zone of schists, both more or less interrupted, an association closely resembling that so common in the Alps. The facts displayed warrant the conclusion, previously advanced by the author, that these older rocks of Southern Italy, with the isolated portions observed on the western coast, are the continuation of the Alps by Genoa, while the western side of the peninsula is to be regarded as an immense area of subsidence.

A careful discussion follows of the important earthquake phenomena which have been observed in Southern Italy. The main conclusion reached is, that the earthquake-shocks of Sicily and Calabria may be separated into three classes:

1. Eruptive-shocks, having their center in a volcano, and involving in general only the immediate region about the mountain, such as have often accompanied the eruptions of Mt. Etna.

2. Radial shocks, which also have their origin in a volcano, but are sent out from it along definite lines. Examples of these are afforded by the many earthquakes which have come to Southern Italy from the Lipari Islands.

3. Peripheral shocks, those which have not a volcano as their center, although there may be a certain connection between them and neighboring volcanoes. The line of these shocks may be traced from Cosenza southwest through Oppido and Reggio, and is probably continued through Mt. Etna toward Palermo, thus forming a complete semicircle concentric with the Lipari Islands. These shocks differ from the radial shocks in that they are propagated forward or backward on this line, so that they may visit the same place within a short time from different directions.

Another line of shocks is shown to have the direction from Mt. Vultur southwest through Potenza to Papasidero.

From the facts presented, Professor Suess shows that there is a close connection between the volcanoes and the earthquakes. He concludes, moreover, that the volcanic phenomena of the Italian peninsula are parts of one system, and that they are to be regarded not as the cause of the mountain elevation, but as accompanying phenomena resulting from fractures that have been produced through other means. E. S. D.

12. *Distinctive optical properties of the Feldspars.*—DESCLOIZEAUX has recently investigated the optical properties of the triclinic feldspars (C. R., lxxx, Feb. 8, 1875). The principal results obtained by him are contained in the following table, in which *Bx* stands for *bisectrix*.

	ALBITE.	OLIGOCLASE.	LABRADORITE	ANORTHITE
Acute bisectrix	always +	generally − sometimes +	always +	always −
Angle made by the + Bx. with a normal to *i-ĭ* (*g*)	15°	18° 10′	30° 40′	Position of the Bx. has no simple relation to the planes observed on the cryst'ls.
Same, with a normal to *O* (*p*)	78° 35′	68°	56°	
Angle made by the line in which the plane of the optic-axes cuts *i-ĭ*, with edge *i-ĭ* | *O*(*g*′|*p*)	20°	Line parallel to the edge *O* | *i-ĭ*.	27°–28°	
Same, with edge *i-ĭ* | *I*(*g*′|*m*)	96° 28′(front)	" "	37°25′–36°25′	
Ordinary dispersion	$\rho < v$ (+ Bx.)	$\rho < v$ (+ Bx.)	$\rho > v$ (+ Bx).	$\rho < v$ (− Bx.)
Dispersion parallel or perpendicular to plane of polarization	*Inclined;* probably also slight *horizontal.*	*Crossed;* also slight *inclined.*	*Crossed;* also slight *inclined.*	*Inclined.*
Appar'nt optic-axial angle (in air)				
—— for red rays	80° 39′	89° 35′	88° 15′	84° 58′
—— for blue rays	81° 59′ (Roc tourné)	88° 31′ (Sunstone. Tvedestrand)	87° 48′ (Labrador)	85° 59′ (Somma)

The axial divergence is quite constant for albite, labradorite and anorthite, but varies for oligoclase even in different sections taken from the same specimen. DesCloizeaux concludes from his observations that labradorite and oligoclase have an equal right with albite and anorthite to be considered independent species, contrary to the views presented by Tschermak. Andesite he concludes to be altered oligoclase, while tschermakite is identical with albite. E. S. D.

13. *Insects and other fossils from the Permian of Weissig in Saxony.*—Mr. EUGENE GEINITZ, in a second paper on the carbonaceous shale of the Permian at Weissig, near Pillnitz, mentions the occurrence there of four species of Blattina, one a nearly perfect specimen of the *B. didyma* Germar, another a new species, the *B. porrecta?* Geinitz, and the others the *Blattina Carbonaria* Germar and the *B. Mahri* Goldenberg. The same locality has afforded also, *Uronectes* (*Gampsonyx*) *fimbriatus* Jordan, *Estheria tenella* Jordan; the fishes, *Acanthodes gracilis* Beyrich, *Palæoniscus angustus* Ag., *P. Vratislaviensis* Ag.; the Articulate, *Gyromyces Ammonis* Göpp. (*Spirorbis Carbonarius*); and several plants of the genera

Annularia, *Asterophyllites*, *Sphenopteris*, *? Hymenophyllites*, *Odontopteris*, *Neuropteris*, *Dictyopteris*, *Cyatheites*, *Alethopteris*, *Tæniopteris*, *Walchia*, besides some others.—*Jahrb. f. Min.*, 1875.

14. *Batrachians in bituminous shales of the Permian at Millery, France.*—M. A. Gaudry states that a single slab of shale from Millery, France, has on it seven little Batrachians, resembling the species which had been obtained by M. Roche at Igornay. He names the species from the two localities *Salamandrella Petrolei*, because of the affinities to the Salamanders, and their occurrence in oil-yielding shales. They are very small, the largest 35 mm.; and yet they were probably adults. No traces of scales were found. The fore and hind limbs are four-toed and nearly of the same size.—*Ann. Mag. N. H.*, IV, xv, 233.

15. *Coal in Patagonia.*—On the Peninsula of Brunswick, in the Straits of Magellan, at a place called Vaqueria by Capt. Corey, rich beds of coal have been opened. The place is not far from the Chilian Colony of Punta-Arenas, lat. 53° 10′ S. and long. 70° 54′ W. (from Greenwich). The Chilian Government has conceded it to a French company. A detailed report has been published by M. F. Arnal, Civil Engineer. The coal is very compact, black, inflames easily and burns without odor. There are three beds, having an aggregate thickness of about 26 feet. The age of the beds is not stated; but as the coal is spoken of as related to the lignites, it is probably Tertiary or Mesozoic.—*L'Institut*, Feb. 17.

16. *Icones Muscorum, or Figures and Descriptions of most of those Mosses peculiar to North America, which have not yet been figured;* by the late WILLIAM S. SULLIVANT, LL.D. *Posthumous Supplement;* with 81 copper plates. Imp. 8vo. Cambridge: Charles W. Sever. London: Trübner & Co. December, 1874.—To give an idea of the nature of this volume, and of the circumstances under which it has been produced and published, we append the brief preface, written by the author of this notice. His biographical sketch of Mr. Sullivant, published by the American Academy of Arts and Sciences, is prefixed to the volume. The mosses which form the subjects of the present series of incomparable plates, are partly Californian, from the indefatigable Bolander and others, or from the Rocky Mountains, &c., and partly new discoveries in the Atlantic States, by Messrs. James, Austin, Peck, Ravenel, &c. Only a small edition is printed and put on sale. Its price, as of the original volume, is fixed—at least for the present—in reference to the ordinary means of students and devotees of Bryology, rather than to the actual cost of the work. Even if all the copies were sold at the price ($10 for this supplement, and $14 for the original volume, which is still to be had), the gift of the lamented author to his fellow-botanists would yet be large. If desired, copies will be supplied from the Herbarium of Harvard University.

"Since the publication of the Icones Muscorum, in the year 1864, the lamented author made careful studies and sketches of the copious new materials as they came to his hands, with a view to a

continuation. A portion of these was selected for the first Supplement, and the drawings and engravings of these eighty-three plates were completed before his death, in April, 1873. The drawings were made under his immediate superintendence, by the same meritorious draughtsman who executed those of the original volume, Mr. August Schrader, and they were engraved on copper by Mr. William Dougal. Some progress had been made in the preparation of the letter-press, but only a small portion of it was found to be in readiness for the printer. Its preparation and completion, as well as the oversight of the press, have devolved upon his friend and intimate companion in Bryological studies, Mr. Leo Lesquereux, to whom this has been a labor of love and the paying of a tribute of affection to an endeared memory. Inasmuch as he had to draw up a large proportion of the following descriptions from mere notes and remarks appended to the specimens in the herbarium, it would have been only just, no less to Mr. Sullivant than to Mr. Lesquereux, that the name of the latter should be placed upon the title-page as the editor of this posthumous work. But he has strenuously objected to this, and his decided wishes have been deferred to. Regarding the incomparable "Icones" and this Supplement as a fitting memorial of one who will rightly be remembered as the father of American bryology, he declines to have his own name inscribed upon the monument, however subordinately. Accordingly, it only remains for the writer of this preface to express—on the part of the botanists who are to be benefited, and of the Sullivant family, at whose expense this memorial is published—most grateful acknowledgements to Mr. Lesquereux for his valuable and essential services." A. G.

17. *Attar of Roses*, for European commerce, is almost exclusively supplied by a small tract of country in Rumelia, on the southern side of the Balkan Mountains. There has been some question as to what species of Rose was employed; but of late it has generally been thought to be the Damask Rose. Mr. Hanbury, having received a packet of specimens from the rose fields of the Balkan, submitted them to Mr. Baker of Kew, he being the most accomplished rosarian; and Mr. Baker has determined them to be for the most part *Rosa damascena*. This species is quite unknown in a wild state; and Mr. Baker regards it "as most likely a cultivated race of *R. Gallica*, which spreads in a wild state from France to Kurdistan." (See Jour. Bot. for Jan., 1875.) A. G.

18. *British Wild Flowers considered in relation to Insects;* by Sir John Lubbock, Bart, F.R.S., M.P., with numerous illustrations. London: Macmillan & Co. 1875. pp. 186, 12mo.—Although the revival of this fascinating and fruitful subject, and the first clear exposition of its importance in the vegetable economy, are due to an Englishman—it being Mr. Darwin's happiest piece of subsidiary work—there has been no British text-book or popular treatise* to impart the requisite knowledge of the facts, and to

* Except a little work by Mr. Bennett, entitled "How Flowers are Fertilized," which we have not seen.

excite and gratify the interest which these are sure to engender. There have been some excellent articles in the magazines; but Mr. Darwin's revelations seem not to have been followed up as they have been elsewhere, especially in Germany, where the literature of the subject is very considerable. In the United States it has become a part of popular botanical instruction,—not so generally, however, as it ought to be; for this, and other matters of plant-behavior, may be expected to double the charm of botanical study, by opening new fields of surpassing interest and almost endless variety. Nothing can be better adapted to sharpen the powers of observation in the young.

Sir John Lubbock's lecture before the British Association last August upon this topic attracted great attention, which his series of articles in *Nature* widened; and now these articles are collected to form this handy volume,—one of the *Nature Series*, to which the author had already contributed his more elaborate and thorough essay on "The Origin and Metamorphoses of Insects." As the author remarks, he "had made no serious study of Botany," at which we do not wonder. As it is, few indeed can be expected to give their powers, with the success he has commanded, to so many engrossing subjects, financial, ethnological, zoological and other. But as an entomologist, "the intimate relations which exist between flowers and insects," as brought to light by recent researches, came directly in his way. His observations and notes originally prepared—and as we judge very recently—"with the view of encouraging in my children that love of natural history from which I myself have derived so much happiness" (and we may add science no little advantage), naturally suggested that a wider use might be made of them; and this attractive, but rather hastily prepared volume is the result. Not much in the way of original observations is here to be expected; but there are some, and good ones. There are seven chapters. The two introductory chapters are the most important and satisfactory. They give an admirably clear account of the general subject, succinct, but sufficiently full to convey a good idea of the various ways in which most flowers are adapted to be cross-fertilized by the visits of insects, and through which these visits are secured, while others are in their way as well served by the winds. And there is an especially instructive account of the modifications of the mouth-parts and legs of bees and butterflies, for their profit in their visits to flowers, while they profit them. This is condensed from Herman Müller and the illustrations are his.

In the remaining chapters the orders of the principal British flowers are taken up seriatim, and the fertilization of a good number of them explained and illustrated,—sufficiently so in many cases, at least in the earlier orders; but, on the whole, if so wide a range and systematic treatment were undertaken, it were better to carry it out more fully. The little cuts from Bentham's Handbook, not having been prepared in view of this subject, are here of small use. They are supplemented by larger and clear

illustrations of separate flowers, from Müller, Axell, Hildebrand and Darwin.

Now that we fairly well understand the meaning of these various arrangements for the fertilization of flowers, perhaps we ought to give to the first discoverer of them, Christian Conrad Sprengel, rather more credit than the present author awards him. If he did not perceive "the real significance of the facts which he had discovered," it was not because he, like his contemporaries, was "led to assume that each flower was created as we now see it." The meaning of the arrangements comes out as clearly upon that supposition as upon any other. Mr. Darwin puts the case better, when with his characteristic exactness, he says that "Sprengel failed to understand the full meaning of the structure of the flowers which he has so well described, from not always having before his mind the key to the problem, viz: the good derived from the crossing of distinct individual plants." The law of nature that organic beings shall not fertilize themselves in perpetuity, is the key to the whole; and for this key we rightly acknowledge our indebtedness to Mr. Darwin, who first turned it to full account, in a masterly way. Yet the principle involved was enunciated by Andrew Knight, in the year 1799. But even this was anticipated by Sprengel, in 1793, in the declaration to which his observations had led him, that "Nature does not wish that any complete flower should be fertilized by its own pollen." Kœlruter's equivalent suggestion is still earlier, and much antedates that of Knight (instead of following it, as Mr. Knight implies), if really to be found in his book published in 1761, as is lately stated. Like most first discoveries, these were not followed up, nor half their importance foreseen. A. G.

19. *Amphipod Crustaceans of the family Gammaridæ in Lake Baikal.*—Dr. B. N. Dybowsky has described *ninty-seven* species of Gammarids from Lake Baikal. They include one Swedish species, *G. (Pallassa) cancelloides;* and also the *G. neglectus* of the lake is hardly distinct from *G. pulex.* The species occur at all depths, the greatest depth dredged, 1373 meters, affording them as abundantly as the littoral zone, though fewer in species. The species of small depths are mostly vividly colored; those at greater depths are less bright in color, and the kinds from depths greater than 700 meters are more or less whitish in tint.—*Ann. Mag. Nat. Hist.*, IV, xv, 230.

20. *Large Cephalopod.*—A cephalopod "of great bulk," 12 feet in length, has been captured on the coast of Japan.—*Japan Mail*, Jan. 23.

21. *The Doctrine of Descent and Darwinism;* by Oscar Schmidt, Prof. Univ. Strasburg. The International Scientific Series. 324 pp., 12mo, with 26 wood-cuts. New York, 1875. (D. Appleton & Co.)—This volume contains, in a brief dogmatic form, the views of Darwin on descent, combined with those of the more imaginative Hæckel. The author has nothing but matter in his philosophy, and is intolerant toward those who do not think with him.

III. Astronomy.

1. *Venus.*—The Italian observers at Maddapore in Bengal, to which party the eminent spectroscopist Tacchini belonged, besides observing all four contacts, ascertained an important fact respecting the atmosphere of Venus. The ring around the planet, which in the former transits, as in the present one, was visible around Venus both on and off the sun, indicates in the spectroscope that the atmosphere contains aqueous vapor.—*Nature*, Jan. 21.

2. *A Society for Spectroscopic observations on the Sun* has been formed in Calcutta.—*Nature*, Feb. 25.

3. *Astronomical and Meteorological Observations made during the year* 1872 *at the U. S. Naval Observatory.* Rear-Admiral B. F. Sands, U. S. N., Superintendent. Published by authority of the Hon. Secretary of the Navy. cvi, 372, 58 and 22 pp., 4to. Washington, 1874.—This new volume of the U. S. Naval Observatory opens with an account of the Observatory, the Transit Circle, with tables for correction and use, etc., and the Mural Circle, occupying over 100 pages. The following part of the volume contains the tables of the results of observations in 1872 with the Transit (160 pp.); with the Mural Circle (161–204 pp.); with the Equatorial (207–232 pp.); mean declination of stars, from individual observations with the Mural Circle (235–246 pp.); mean places of miscellaneous stars, from individual observations with the Transit Circle (249–268 pp.); Catalogue of stars observed with the Transit Circle (271–278 pp.); Right ascensions, N. Polar distances and semi-diameters of the Sun; Small Planets; Declinations of the Moon. It concludes with meteorological tables, and a report on the difference of longitude between Washington and Detroit, Michigan, Carlin, Nevada, and Austin, Nevada.

4. Professor Argelander.—Friederich Wilhelm August Argelander, born at Memel in East Prussia on the 22d of March, 1799, died at Bonn on the 17th of February. He was for a while assistant in the observatory at Königsberg, under Bessel, afterward, in 1823, Director of the Finland Observatory at Abo and Helsingfors, and in 1834, Director of the Prussian Observatory at Bonn, where he continued his assiduous labors until within a short time of his death.

Professor Argelander's astronomical labors were principally in the field of fixed-star observing. In this they may be described as Herculean; his survey of the northern heavens including the observation in zones of more than *three hundred thousand* stars, the charting of which has been of the greatest service in many astronomical inquiries. Nor did he fail to compare many of his observations, being thereby led to several remarkable discoveries of proper motion and variability of light of certain stars. While in Finland, he made a determination of the motion of the solar system in space, with results nearly the same as those of Sir William Herschel. His careful and comprehensive estimation, too, of the comparative magnitudes of all the stars visible to the naked eye should be mentioned (Uranometria Nova).

The Royal Astronomical Society of London marked their sense of the value of Professor Argelander's sidereal labors by the award of their gold medal in February, 1863. His last observations, so far as we are aware, were of the bright comet of Coggia, in the summer of last year.—*Athenæum*, Feb. 27.

IV. Miscellaneous Scientific Intelligence.

1. *Engineer Department U. S. Army. Progress-Report upon Geographical and Geological Explorations and Surveys west of the 100th Meridian in* 1872, under the direction of Brig. Gen. A. A. Humphreys, Chief of Engineers, U. S. A.; by First Lieut. George M. Wheeler, Corps of Engineers, in charge. 56 pp., 4to. Washington. 1874.—According to this report, the work performed by the expedition under Lieut. Wheeler in 1872 extended over the astronomical, topographical, meteorological, geological and natural history departments. For the determination of latitude and longitude three parties were engaged, and for topographical observations, five parties. The area covered topographically during the year exceeded 50,000 square miles, and included part of Western and Southwestern Utah, Eastern Nevada, and Northwestern Arizona. This "Progress-Report" contains historical and descriptive accounts of various mining districts and mines within the region examined, observations on irrigation for the country, and on its agricultural resources, timber lands, routes for communication, and other points of interest.

The geologist, Mr. G. K. Gilbert, states that about White's Peak, in the Schell Creek Range, Nevada, in latitute 39° 15′ north, there are the terminal moraines of five or six glaciers, 8,000 feet above the sea-level; on Wheeler's Peak, of the Snake Range, Nevada, latitude 39° north, several moraines and an alpine lake; and on Old Baldy Peak, near Beaver, Utah, in latitude 38° 18′ north, two terminal moraines. The ancient extent of the great Salt Lake,—when it stood 900 feet above its present level as proved by its elevated beach lines,—is made 18,000 square miles, which is a little less than the area of Lake Huron. Mr. Gilbert observes that, in the plateau region of the Upper Colorado, the rock-system, ranging from the Tertiary to the Devonian, is comparatively undisturbed, and denudation has left the harder beds projecting in a succession of steps. Across these steps there is a series of parallel faults which divides the steps into limited tables, that are drained by tributaries of the Colorado and Sevier Rivers.

According to the scheme for the publication of the results of the Wheeler Expeditions, the Report will include six quarto volumes, and also one topographical and one geological atlas, each 19 by 24 inches. Volume IV of the series is to contain the geology; volume V the paleontology, which is to be illustrated by numerous plates of vertebrate and invertebrate fossils; and volume VI, the natural history of the expeditions.

Numerous excellent photographs have been taken, which will be used for illustrating the geological and other volumes. They

represent grandly the scenery of the Colorado and of other parts of the region under survey.

2. *Smithsonian Contributions to Knowledge*, 1874, 4to. *Miscellaneous Collections.* Vols. xi, xii, 8vo, 1874.—The very valuable series of Smithsonian publications was enlarged by the addition of three volumes during the year 1874. The volume of Contributions contains Problems of Rotary motion presented by the Gyroscope, the Precession of the Equinoxes and the Pendulum, by Brevet Maj. Gen. J. G. Barnard; a contribution to the History of the Fresh-water Algæ of N. America, by H. C. Wood, Jr., M.D., with 21 plates, many of them colored; an investigation of the Orbit of Uranus, with general tables of its motion, by Simon Newcomb.

Vol. xi, of the Miscellaneous Collections, contains memoirs on the Arrangements of Mammals and Fishes, by Dr. T. Gill (200 pp.); monographs of N. A. Diptera, by H. Lœw (376 pp.); on collecting and preserving Insects, by A. S. Packard, Jr. (60 pp.); New N. A. Coleoptera, and classification of the Coleoptera, by J. L. LeConte (146 pp.).—Vol. xii contains a Review of American Birds in the Smithsonian Museum, by S. F. Baird (484 pp.); the Constants of Nature, Part I: Specific gravities, Boiling and Melting Points and Chemical Formulæ, by F. W. Clarke (272 pp.); on Telegraphic Announcements of Astronomical discoveries, by Joseph Henry (4 pp.).

3. *Chemical Examination of Alcoholic Liquors;* by Professor A. B. Prescott. 108 pp., 12mo. New York. 1875. (Van Nostrand.)—Professor Prescott has supplied a want long acknowledged in our chemical literature by this concise and well arranged little manual. The special acts of legislation on the subject of alcoholic liquors and wines, their adulteration and falsification, which now slumber in the statute books of this country, have been framed with so little skill or with such bigotry or partizanship as to be practically inoperative. The importance to society that all articles used as foods, medicines, or beverages should be held subject to strict scientific examination by authority of law, and that the frauds now practiced should be systematically exposed and suppressed, is such as to admit of no debate. These frauds, pernicious to health or life, are by no means confined to alcoholic beverages. And the analyist will find assistance in meeting them, not only in this Manual of Dr. Prescott, but also through the use of the Outlines of Proximate Organic Analysis, by the same author, already noticed. B. S.

4. *Appleton's Cyclopedia.*—The American Cyclopædia: a Popular Dictionary of General Knowledge. Edited by George Ripley and Charles A. Dana. Vols. I—X, 1873–1875. Royal 8vo. New York.—It is now twelve years since the first edition of Appleton's American Cyclopedia was finished. It was followed in the eleven succeeding years by as many "Annual Cyclopedias," uniform with the original work, in sixteen volumes. Although much the larger part of these volumes is properly given to historical, political, civil, military and social affairs, including biography,

statistics, commerce, finance, and literature—science has by no means been neglected, as we have in successive years pointed out in our bibliographical notices.

The new edition of this work, of which ten volumes are now ready, was undertaken in 1873. No decade of this century has been marked by more rapid advances in science than that covering the interval between these two editions; and equally remarkable have been the social and political changes. To fuse all the new material, thus rapidly accumulated, with the old, into a homogeneous and compact unity, has required the recasting of the whole work. Of course a large number of writers and revisers have been busy in preparing the eight or nine thousand closely printed pages in the ten volumes already published. Many of these articles are brought down to date, with a completeness not found in the first edition; while, as far as we have examined the several volumes, the matter of former scientific and technical articles has been extensively revised with care and good editorship. The geographical maps introduced add greatly to the value of the Cyclopedia, and the wood-cut illustrations are numerous and well selected. A considerable number of names of authors, not before contributors to the Cyclopedia, now appear in connection with articles on various scientific topics. The authorship of some of the physical, chemical, geological, and technical articles is as follows: Count L. F. de Pourtalès, of the U. S. Coast Survey, Prof. J. E. Hilgard, Dr. Thomas M. Drown, Prof. Cleveland Abbé, Dr. Rossiter W. Raymond, Dr. T. S. Hunt, Dr. A. M. Mayer, Dr. Joy, and Dr. Charles L. Hogeboom. Many of the astronomical articles are from the pen of Mr. Richard A. Proctor, of England.

The tenth volume ends with "Magnet," by Cleveland Abbe.

B. S.

5. *Transactions of the American Institute of Mining Engineers.* Vol. ii. May, 1873, to Feb., 1874. 331 pp. 8vo, with 3 plates. Easton, Pa. Published by the Institute, at the office of the Secretary, Lafayette College.—This vigorous young society is doing good service in its published contributions of original memoirs on subjects appropriate to its purposes, many of them of permanent value, recording the results of methods and investigations of great interest to all concerned in such matters. B. S.

6. *U. S. Northern Boundary Commission*, A. CAMPBELL, Commissioner. *Natural History*, No. 1. *On the Muridæ*, by Dr. E. COUES, U. S. A., Surgeon and Naturalist of the Commission. Reissued with additions from the Proc. Acad. N. Sci. Philad., 1874. 28 pp., 8vo, 1874. Contains many valuable notes on the species.

Birds of the Northwest: a Handbook of the Ornithology of the region drained by the Missouri River and its Tributaries; by ELLIOTT COUES, Capt. and Assist. Surgeon U. S. A.—Miscellaneous publications 3, of U. S. Geol. Survey of the Territories, F. V. HAYDEN, U. S. Geol. in Charge. 792 pp. 8vo. Washington, 1874.

Texas Geological Report.—Mr. Buckley, in a recent letter to the editors, states that the heading of Devonian in his Report over a section treating of the Trenton formation (see p. 224 of this volume) was a printer's error. There is nothing in the Report to suggest this.—EDS.